The Long-Term Effects of School Quality in a Low-Income Country: Evidence From 15 Years of Data

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Abstract

Using 15 years of rich data following Pakistani children, we causally estimate the effects of attending a better primary school on children's long-term outcomes. Panel test score data allow us to estimate value-added measures of school quality. Exploiting variation generated by school switches, typically driven by moves to keep siblings in nearby schools, we show that on average being exposed to a 1 standard deviation better school increases adult test scores by 0.2 sd, total years of schooling by nearly 1 year (12%), the likelihood of graduating high school by 10 ppt (51%), and college enrollment by 5 ppt (48%). Hence, the effects of attending a better primary school do not fadeout by adulthood. In addition, female labor force participation nearly doubles. Two additional identification strategies, including a "movers-style" strategy replicating the approach in the mobility literature, deliver quantitatively similar results. Our estimates of the long-run effects of foundational learning on educational attainment in combination with Mincerian estimates of the returns to education – which closely mirror our causal estimates for effects on male wages – can be used to quantify the effects of interventions on learning in low-income settings in terms of earnings. For example, moving only the current cohort of male children from the worst school to the best school in the average village would generate present discounted wage gains of 302,000 USD (216 times GDP per capita).

1 Introduction

For more than fifty years, dating back to at least Mincer (1974), economists have sought to causally estimate the returns to an additional year of schooling. At the same time, there is growing evidence that school quality is highly heterogeneous, even within extremely small geographic areas (Andrabi et al., 2025). An additional year of education at one school may have very different implications for long-run outcomes than an additional year at another. Understanding the effects of school quality is especially important in low-income settings

because the magnitude of these effects relative to estimates from high-income countries is theoretically ambiguous. On the one hand, foundational learning could have especially high returns in these settings both because curricula are often too advanced for students and do not allow for catch-up (Muralidharan et al., 2019) and because parents believe there are higher returns to investing in children who are already doing well and therefore re-enforce early differences in cognitive skills (Dizon-Ross, 2019). On the other hand, a growing literature suggests that early gains to cognitive skills fadeout (Bailey et al., 2017), implying that even large differences in school quality early in life could become unimportant by adulthood. Fadeout may be particularly important in low-income contexts where children are often in and out of school to assist households and parents are less able to make complementary investments in skills.

As policymakers in low-income countries decide whether to dedicate limited resources to increasing school quality relative to other competing priorities, understanding the returns on these investments is critical. As of 2012, low and middle-income countries spent approximately 11.5% of GDP per capita per primary school student (World Bank, 2025b). However, estimating these returns in the lowest-income settings is also particularly challenging. There are *no* detailed administrative data on test scores, allowing for the estimation of school value-added (SVA) – the standard measure of school quality that captures the predicted effect of attending a given school on a student's annual test score gains – much less administrative data that allows for linking SVA to long-run outcomes.

This paper exploits a unique study that followed children in Punjab, Pakistan for 15 years to provide the first causal estimates of the effect of primary school quality on long-run outcomes, including cognitive skills, socioemotional skills, functional literacy/numeracy, educational attainment and employment. Between 2003 and 2007, we collected panel data on primary school students' test scores, allowing for the estimation and validation of SVAs, as described in Andrabi et al. (2025). In this paper, we match these SVA estimates with information on long-run outcomes collected for a subsample of children in subsequent surveys in 2011 and 2018. While this long-running data collection resolves the challenges posed by the lack of data on school quality linked to adult outcomes, a second challenge arises from the fact that children select into schools. Though our measures of SVA are unbiased, since as in higher-income countries, richer children typically attend higher SVA schools, simply regressing outcomes on SVA will still lead to biased estimates of the effect of school quality.

We address this selection problem with multiple identification strategies. In our primary strategy, we compare students who are first observed in the same school and exploit variation in the average SVA experienced by these students due to school switches. Intuitively, first school fixed effects control for active school selection based on quality, akin to the lagged

test score control in value-added models, and the validity of the approach relies on the fact that subsequent switching is conditionally unrelated to omitted variables like ability or socioeconomic status. Consistent with this, the vast majority of switching appears to be driven by parents moving siblings to keep them in close together schools. This accords with several features of the environment. Parents have multiple school-age children (the average child has 3 siblings, in line with the total fertility rate in Pakistan at this time), roughly half the switches we observe are between nominally single-sex public schools, switching does not result in any *average* change in school value-added, and the plurality of parents (43%) report choosing a school based on distance. We show that the data are consistent with the identifying assumptions that switchers are no better or worse than stayers, that students who switch into better schools are not different in levels or in terms of trajectory, and that there is no differential attrition from the sample.

Experiencing a 1 sd higher average SVA during primary school leads students to complete 1 more year of schooling. As adults, in 2018, students who experience 1 sd higher average SVA in primary school have higher cognitive skills (by 0.2 sd) and higher literacy and numeracy (0.3 sd), limiting the scope for fadeout. In contrast, their non-cognitive skills are unchanged. The increases in educational attainment are driven by higher rates of attainment at almost every level. Students who attend better schools are more likely to complete 10th grade (approximately 10 percentage points or 23%), receive a high school diploma (10 percentage points or 50%), and go to college (5 percentage points or 43%). The link between school value-added and long-run outcomes is robust to including rich controls for socioeconomic status, focusing on switches to worse or better schools, and focusing on the most plausibly exogenous switches (those concurrent with a sibling switch where the two children are *not* switching between the same schools).

We complement the primary strategy with two additional approaches. First, we closely follow the "movers strategy" in Chetty and Hendren (2018), restricting our sample to switchers and comparing those who spend more vs. less time in a higher quality school. The findings are quantitatively similar to those from the primary strategy, and the estimates show that the effects of moving to a better school on years of education grow monotonically with the number of years spent in that school, in line with school quality causally affecting long-run outcomes. Second, we report results from a lasso instrumental variables strategy exploiting households' high degree of distance sensitivity in this context.[1] Importantly, Andrabi et al. (2025) show that conditional on distance to the center of the village, a household's location is unrelated to household characteristics. To exploit variation in the quality of nearby

[1] See Bau (2022) and Carneiro et al. (2024) for evidence on the distance sensitivity of enrollment decisions in rural Pakistan.

schools (and hence a household's effective choice set), we propose as potential instruments the average SVA of schools within varying radii around the household. The lasso regression selects the instruments that are most predictive of the SVA of the school a student actually attends. We then verify that these instruments, like the location instrument in Andrabi et al. (2025), are unrelated to household or student characteristics conditional on distance to the center of the village. This strategy allows us to estimate the effects of school quality using totally different variation than our other two strategies. While the first two strategies rely on variation in the schools students attend over time due to the panel structure of the data, this strategy exploits cross-sectional variation from students' locations. While less precise, the IV-lasso results are qualitatively similar to those from the other strategies, further strengthening the causal interpretation of our results.

We consider several possible channels for the link between high SVA primary schools and educational attainment. While the most straightforward explanation is that better schools improve cognitive skills, and this in turn leads to greater educational attainment, there are several other potential mechanisms. One possibility is that higher quality schools have better reputations and therefore provide a more positive signal of student quality, which helps with admissions at the secondary/college-level (MacLeod and Urquiola, 2015; Sekhri, 2020). To evaluate whether this is the case, we include an (endogenous) control for peer quality in our regressions. The effects on educational attainment are robust, suggesting that school quality itself rather than peer quality matters. Alternatively, better quality schools could help students pass hurdle exams with high fail rates. However, this is inconsistent with the data: attending a better quality primary school has large effects on educational attainment even before students reach the first hurdle exam in 10th grade. Another possibility is that the effects are driven by attending a private school rather than a higher SVA school per se; this is an important potential confounder since private schools have higher SVA on average in this context (Andrabi et al., 2025). We directly control for private schooling exposure and show that the effects remain even after accounting for this correlation. Higher SVA schools could also be more likely to offer higher grades, reducing switching costs, but we again find no evidence that this explains our results. Finally, it could be that schools that produce more cognitive skills also produce more non-cognitive skills, and it is these skills that lead to more educational attainment (Heckman et al., 2013; Jackson et al., 2020). However, we find no evidence that higher SVA schools improve non-cognitive skills more in this context.

In the last part of the paper, we turn to the longer-run impacts of school quality on employment and wages. This is a more challenging question because a large portion of our sample is still in school (their ages range from 17 to 32), and this is causally higher among those who attended higher quality primary schools. As the wage gains from attending

higher quality primary schools are likely to be highest among those who ultimately attain the most education, any estimates that condition on employment are likely to be downwardly biased. Even among those who are currently working, the less-educated are mechanically more experienced, which may lead their wages to look relatively higher in the early career period when we observe participants. Chetty et al. (2014a) observe a similar pattern when measuring the effects of higher quality teachers on their former students, estimating initially negative effects on younger adults that turn strongly positive as they age.

With this in mind, we report suggestive estimates of the effects of attending a better primary school on employment and wages. Reflecting men and women's very different labor force participation rates – employment is close to universal for male non-students, while only 7% for women in our sample – the story varies by gender. For men, there is a strong decline in working, but this is explained by the fact that men are much more likely to still be in school. For women, on the other hand, while enrollment also appears to increase, because baseline labor force participation is so low, this does not lead to a strong decline in labor force participation. In fact, on net, a woman who attends a 1 sd better primary school over the course of her education is 5 ppt (≈70%) more likely to work, perhaps because education gives her access to more socially acceptable teaching jobs. Our results suggest that improved foundational learning can play a role in opening up labor market opportunities to women, even in a highly gender-biased society.

Cautioning that our wage estimates are likely to underestimate the long-run effects of attending a higher quality school for the reasons mentioned above, in our sample of working men, we estimate that attending a 1 sd better school increases wages by 10% (albeit not statistically significantly). This closely matches what we would predict if the full wage effect were due to increased education, since attending a 1 sd better school leads to 1 more year of education, Mincerian regressions suggest that the private return to a year of schooling globally is 9% (Patrinos and Psacharopoulos, 2020), and Duflo (2001) causally estimates a similar wage return in Indonesia. For completeness, we report results for working women, where the effects are large and positive, but given the extremely small sample size, these estimates should be interpreted with caution.

The similarity between our estimates of the increase in wages and estimates of the Mincerian returns supports the implementation of a surrogate approach in the style of Athey et al. (2019), where we link the learning gains we observe to their effects on long-run outcomes and then estimate the effects on wages based on the Mincerian returns. This allows us to provide a likely lowerbound – given that we do not expect all the returns of improved learning to manifest via increases in education – quantification of the benefits of moving children across schools. More broadly, our estimates of the link between primary school learning

and educational attainment, linked to the Mincerian returns, can be used to assess the cost-effectiveness of learning interventions in low-income countries in terms of long-run wage effects. To our knowledge, without the evidence on the long-run link between learning and educational attainment provided in this paper, such calculations would require researchers to make heroic assumptions about the causal effect of learning on educational attainment or wages.

This paper contributes to two related literatures. First, it contributes to a growing literature documenting the importance of and estimating the effects of school quality. In the context of charter schools in the United States, Hoxby and Murarka (2009) and Angrist et al. (2013) show that school quality is variant and that this has important implications for learning in the short or medium-term, while Andrabi et al. (2025) document that this is also true even in small Pakistani villages with an average of 678 households. A smaller literature mainly in higher-income contexts investigates the longer-run impacts of school quality, but this literature faces many of the same challenges as in low-income settings. To causally estimate the long-run effects of school quality, researchers need both data that links school quality to long-run outcomes *and* a source of quasi-experimental variation in the schools students attend. Because of these challenges, much of this literature focuses on a small number of oversubscribed charter schools, where lotteries are available to provide identification (Dobbie and Fryer Jr, 2015; Angrist et al., 2016; Clark et al., 2015) or reports associations rather than causal estimates of the link between SVA and value-added for longer-run outcomes (Jackson et al., 2020). One exception is Beuermann et al. (2023), which exploits exogenous variation to estimate school value-addeds for test scores, arrests, dropout, teen motherhood, and formal labour market participation and shows that these value-addeds are modestly correlated in the context of Trinidad and Tobago, though this setting is five times richer than the one we study. More broadly, while not directly about school quality, Chetty et al. (2014b) measure the long-run effects of teacher quality on students' long-run outcomes. Our paper contributes to the literature by (1) providing the first estimates of the long-run effects of school quality in a low-income setting, (2) documenting effects on cognitive skills – for the first time to our knowledge – more than 10 years after students initially attended a school, and (3) estimating the effects of school quality using the full distribution of schools in a representative sample of villages.

Second, this paper contributes to a still nascent literature on the long-run effects of educational and human capital-oriented interventions in low-income settings. Long-run follow-ups are still rare in development economics, although there are notable exceptions. Araujo and Macours (2021) show that the famous Progresa cash transfer program positively affected educational attainment and labor income expectations in the long-term, while Millán et al.

(2020) measure the long-run effects of a conditional cash transfer program in Nicaragua. Duflo et al. (2024) and Duflo et al. (2023) estimate the long-run effects of secondary school subsidies, and Buhl-Wiggers et al. (2023) estimate the impacts of a literacy program in Uganda 8 years later. In contrast, our paper is the first to our knowledge to estimate the long-run effects specifically of increasing foundational learning in a low-income context. Given the relatively moderate first stage of most educational interventions – Evans and Yuan (2022) estimate that the median intervention increases test scores by 0.1 sd – even in cases where long-run data are available, researchers are likely to be underpowered to estimate long-run effects. In contrast, school quality in Pakistan is highly variant, with the gap in learning between the best and worst school in a village mirroring the test score gap between a high and low-income country (Andrabi et al., 2025). Hence, unusually, we are powered to measure the long-run effects of learning in primary school on later educational attainment.

2 Context and Data

2.1 Context

Education Markets Our study takes place in rural Punjab, Pakistan's largest province. As of 2023, the province had a population of 128 million. This is a context with substantial school choice. At baseline, in 2003, the average village in our sample had 680 households but 7.4 schools (4.4 public and 2.2 private). Students choose between public schools, which are nominally single-sex, and private schools, which are typically co-educational. Even though public schools are officially single-sex, a large fraction (30%) report having children of the opposite sex enrolled. Almost all private schools are for-profit and non-religious (Andrabi et al., 2006). There is substantial heterogeneity in school quality among primary schools, even within the same village (Andrabi et al., 2025). Moving a student from the worst to the best school in her village would increase her annual test score gains by 0.43 test score standard deviations, which is roughly equivalent to average annual test score gains.

Villages also experience substantial churn in both the public and private sectors. In our data, 13% of private schools that were open at baseline in 2003 close by 2007 (4 years later) and 12% of schools open in 2007 did not exist in 2003.

Educational Outcomes The period we study is also a transformative one for educational outcomes, though such a transformation is not atypical for low-income settings in recent decades. Our study focuses on children who were enrolled in primary school between 2003–2007. Among these children, about a quarter have mothers with *any schooling*. While

roughly two-thirds have fathers with some primary schooling, very few have parents of either gender that have education beyond primary school. In contrast, 15 years after our baseline round, the average child in our analysis sample has more than 8 years of education, and 11% of men and 15% of women have attended some kind of (typically local) college. Across a generation, individuals have gone from almost no schooling to accessing college at sizable rates. Despite these transformations in human capital attainment, this is not a boom period where we would expect skills to have unusually high returns relative to other low-income settings. Growth fluctuated greatly, but real GDP growth averaged around 4% from 2000-2020. For comparison, India averaged real GDP growth of 6% over the same period and sub-Saharan Africa as a region averaged growth of 4% (World Bank, 2025a). Hence, the Pakistani experience is likely externally valid to other low-income settings.

Gender & Labor Force Participation While Pakistan is generally considered a gender-biased society — in the 2021-2022 UNDP Human Development Report, Pakistan ranked 161st out of 190 countries on the Gender Inequality Index (United Nations Development Programme, 2022) — gaps in educational attainment in our setting are small. In our sample, the average woman completes 8.3 years of education, while the average man completes 8.0 years. At the college-level, 14% of women have attended college, while only 9% of men have. However, men and women have vastly divergent outcomes when we consider labor force participation. In our sample, only 7% of women report working, while 76% of men work. This under-estimates the true difference once schooling is completed, since 11% of men are still enrolled in school at the time we observe them. Once men reach the age of 25, and only a negligible fraction are still in school, the employment rate converges to around 90%. In contrast, for women over the age of 25, employment rates remain below 10% (see Appendix Figure A1). Low female labor force participation rates may reflect the fact that there are few socially acceptable jobs available to women (Heath and Jayachandran, 2018). Indeed, one socially acceptable occupation — teaching —accounts for 65% of employed women in our data.

2.2 Data and Descriptive Statistics

The Learning and Educational Achievements in Punjab Schools (LEAPS) project is a longitudinal dataset on households and schools during which data were collected from 112 villages in rural Punjab, Pakistan. These villages are a stratified random sample of 3 districts of Punjab (Attock, Faisalabad, and Rahim Yar Khan). To be included in the sample, a village had to have a private school at baseline. Hence, these villages are wealthier than the average village but representative of the villages in which approximately 60% of the population lives.

Appendix Figure A2 presents a timeline of data collection. From 2003–2007, all schools in the villages were surveyed, as were a random subsample of 1,807 households. In 2011, five years after the fourth round of data collection, we again surveyed households and all existing primary schools from the 112 original villages. In 2018, we tracked and surveyed some individuals from the 1,807 LEAPS households. This section describes the school and household data collection exercises, focusing on the variables of interest used in this paper.

2.2.1 School Surveys (2003–2007)

Three types of data were collected at schools: test score data from students, socioeconomic information from students, and school characteristics from owners and head teachers. We first describe the test score data and then briefly describe the information on students and schools. The latter type of information is mainly used to construct controls in our analysis and to construct the instrumental variables for the IV empirical strategy.

Test Scores In 2003-04, we tested a first cohort of children enrolled in grade 3 and followed and tested them for four consecutive years. We included a second cohort of grade 3 students in 2005-06, who were tested again in 2006-07 when most students in this second cohort were enrolled in grade 4. Students were tested in math, Urdu (the vernacular), and English. The tests were administered in-school by enumerators. Tests were independent, low-stakes, and designed by the principal investigators. The test scores were constructed over this four year period using Item Response Theory, which results in a distribution of test scores with a mean of 0 and a standard deviation of 1.

We use the test score data from the period between 2003 to 2007 to construct empirical Bayes school value-added (SVA) estimates, our measure of school quality.[2] In Section 3.1, we describe the procedure to construct SVA estimates, which exactly replicates Andrabi et al. (2025). When we construct the SVA measures, we exclude the data of all children who appeared in the 2018-19 long-term follow-up LEAPS data (1,147 individuals or 3,594 child-year observations from the sample of 31,361 tested students or 71,467 student-year observations). Excluding these individuals avoids cases where estimation error leads the estimated SVA to be mechanically related to students' long-run outcomes. This restriction only excludes 1,147 children, even though the 2018-19 survey is considerably larger, because inclusion in the SVA regression, which controls for lagged test scores, requires test scores

[2]While we attempted to test children in the universe of schools, SVA information is missing for some schools. This occurs both (1) because of a small number of school-level refusals (7), and (2) because, mechanically, SVA cannot be calculated if a school existed in 2003 but exited the village in round 2. In the latter case, this is because students are never observed in the school with lagged test scores.

from at least two adjacent years. The majority of our long-run sample, as described below, do not satisfy this restriction.

School Characteristics We surveyed all primary schools in the sample villages and collected socioeconomic information for a random sample of 10 students enrolled in each school. This allows us to calculate measures of peers' characteristics for the students in our study sample, which consist of the average of peers' asset indices, as well as the percentage of peers whose mother and father have at least some primary schooling. During the school data collection, enumerators also collected information about the school type (public, private, NGO, madrassa), GPS coordinates, and grades offered by the schools.

2.2.2 Household Surveys

In our baseline round in 2003, we randomly selected 1,807 households (approximately 16 households in each village), such that in each village, three-quarters of these households had a child enrolled in grade 3, and one-quarter had a child not enrolled in school but at an age eligible to be enrolled in grade 3. This stratification is consistent with the share of eligible children who were actually enrolled in grade 3 at baseline. All resident members of these households were surveyed annually until 2007 and then surveyed again in 2011. In 2018-19, household members who were between 5 and 15 years old in 2003 were tracked for a long-term follow-up survey with the aim of surveying them regardless of whether they had remained in the same household. While this survey was technically not linked to the household, for ease of exposition, we refer to it as a "Household Survey" since the initial sample was identified in the earlier rounds of the household survey. We separately discuss the initial panel between 2003-2007 and the subsequent waves below.

Initial Household Panel (2003-2007, 2011) All six waves of household surveys collected information about socioeconomic status, educational attainment and expenditures, and importantly, between 2003 and 2007, the school and grade attended by all children aged 5 to 15 years old. Thus, we observe which schools children attended for primary school and can match this information to the SVA estimates from the panel test score data. To be included in the final sample for analysis, we must therefore observe a child's enrollment information in the 2003-2007 household panel.

In addition to the enrollment histories in the 2003-2007 household panel, we observe socioeconomic information. This allows us to construct control variables for assets (measured

in the baseline round of 2003),[3] the caste of the household head,[4] and the education of their parents.

The data from the 2011-12 household survey are mainly used to track the evolution of cognitive skills 4 years after the initial data collection ended. Children who were still household members were tested in math, Urdu, and English at home. Hence, even non-enrolled children are included in these data.

Long-term Follow-up Data (2018-2019) Based on the sampling frame of individuals in the baseline household survey who were 5–15, we identified 5,865 potential respondents. We tracked these individuals over the course of two years between 2016 and 2018. When possible, we surveyed them in-person and administered cognitive and non-cognitive tests (75% of the sample). When this was not possible, e.g., because they had left the village and did not return, we surveyed them over the phone. Ultimately, in 2018-19, we surveyed 4,956 individuals for an attrition rate of 15.5 percent, or roughly one percent per year, including individuals who died or could not be surveyed due to security reasons.[5] Appendix Figure A3 shows the age distribution for the final sample.

In both the phone and in-person surveys, we collected information on individuals' educational attainment, wages, and labor market outcomes. In addition, in the in-person surveys, we tested students to collect information on their cognitive and non-cognitive skills, as well as functional literacy and numeracy. Our main outcome of interest is the number of years of education completed, but we also observe whether children completed important milestones in their schooling trajectories, such as middle school, the high-stakes matriculation exam at the end of 10th grade, and a high school diploma at the end of 12th grade.

We measure cognitive skills in math, Urdu, and English using the same tests that were used in the earlier rounds of the LEAPS project. Scores in each subject are constructed following Item Response Theory, as in Das and Zajonc (2010), and we average the subject-specific scores to create a single measure of cognitive skills. Danon et al. (2024) show that there are no ceiling effects from using the same tests seven to fifteen years later. Our measure of non-cognitive skills follows Danon et al. (2024) and is the first component of a principal component analysis (PCA) of multiple measures related to the Big Five personality traits, grit, risk-aversion, and impulse control.[6] Finally, we measure functional literacy and

[3]The asset index is the first component of a principal components analysis of 25 indicator variables for households' assets ownership, normalized for the household size.

[4]We follow Karachiwalla (2019) to determine whether the household head is from a high or low caste.

[5]Four villages in Attock were excluded for security reasons.

[6]The Big Five and Grit are self-reported measures constructed following Laajaj and Macours (2021). This is discussed in detail in Danon et al. (2024) who also use data from the LEAPS project. The risk-aversion measure is determined from the Balloon Analogue Risk Task (BART), a task in which respondents

numeracy with several questions related to everyday tasks such as reading an electricity bill and understanding greeting text messages. The inclusion of these measures is inspired by Banerjee et al. (2025), who show that children who can solve complex real-life math problems are not always able to solve similar or simpler problems in the classroom (and vice-versa). We combine the literacy and numeracy problem-solving questions and again use the first component of a PCA. We refer the reader to Danon et al. (2024) for the list of questions used for each of these measures.

We also collected information on respondents' employment status, whether they still report being in school, and earnings conditional on working. Panel (a) of Appendix Figure A1 reports the percent of men and women in our sample who engaged in an activity for which they have received a payment in the past month. While less than 10 percent of women work, we observe that starting at 24 years old, close to 90 percent of young men work. Panel (b) of Appendix Figure A1 shows the percent of young men and young women who are currently enrolled in school by age. As opposed to employment, both young men and young women display a similar enrollment pattern, where enrollment rates gradually drop from 35 to 10 percent for those aged 18 to 23 years old, and remain at less than 10 percent for individuals older than 24.

2.2.3 Summary Statistics

Our final sample consists of 3,165 individuals in the 2018-19 follow-up survey who were enrolled in a primary school at some point between 2003-07 and whose schools can be matched with an SVA measure.[7] Appendix Table A1 presents summary statistics for key outcome and control variables for the final sample separately by gender.

3 Empirical Strategy

Our empirical strategy consists of several parts. In the first subsection, we describe how we calculate test score SVA. The second subsection describes our primary empirical strategy

earn money from pumping balloons, unless the balloons explode. Finally, to measure impulse control, the respondents had to complete a task on tablets in which they had to touch its screen when it changed color (i.e., the "GoNoGo" task).

[7]The sample does not include 1,536 individuals surveyed in 2018-19 who were never observed enrolled in primary school between 2003–2007 (either because they had already completed primary school, dropped out of school before the sample period started, or because they never attended school). Note that a child progressing "on-time" should complete primary school at age 10 or 11. Another 245 individuals are not included because, even though they were enrolled for at least 1 year, they never attended a school for which we could calculate SVA. This will occur, for example, if the participant's last year of primary school was in round 1, and their school exited in round 2.

and provides evidence in support of the key assumptions. The last two subsections detail the movers and lasso 2SLS strategies.

3.1 SVA Estimation

In order to compute SVA, for math, Urdu, and English, we use the test score panel data from 2003–2007 to estimate

$$y_{igst} = \lambda_g y_{igs,t-1} + \theta_s + \alpha_g^G + \alpha_t^T + \epsilon_{igst}, \tag{1}$$

where i denotes a student, g a grade, s a school, and t a year. The outcome variable y_{igst} is student i's subject test score in year t, λ_g is a grade-specific coefficient that captures the effect of lagged performance, and α_g^G and α_t^T are grade and year fixed effects. Our estimate of a school's subject-specific SVA is the estimate of θ_s, the school fixed effect, for a given subject. Our main school-specific measure of quality is a school's mean SVA across subjects. Equation (1) is the same estimating equation used by Andrabi et al. (2025) and is similar to specifications used by Angrist et al. (2017) to estimate school value-added and Chetty et al. (2014b) and Bau and Das (2020) to estimate teacher value-added.

Intuitively, a school's SVA estimate is the predicted mean annual test score gain for a student who is assigned to a school s. It is unbiased as long as the controls are rich enough to account for the sorting of children into schools. We validate this assumption in Andrabi et al. (2025), where we demonstrate that the SVA estimates are forecast unbiased (they predict the test score gains of a student who switches into a school with coefficient 1) and pass an overidentification test for unbiasedness proposed by Angrist et al. (2017).

While our SVA estimates are unbiased, SVA is still estimated with error. If we include our SVA estimate as an explanatory variable in a regression, estimation error will attenuate its coefficient. To account for this, we use our fixed effect estimates to construct empirical Bayes estimates of the SVAs, as is common in the teacher value-added literature, following the procedure described in Appendix B of Andrabi et al. (2025). We always use the empirical Bayes estimates when SVA is the explanatory variable in a regression.

3.2 School Switcher Identification Strategy

Even if our estimates of SVA are valid and unbiased, students are still likely to sort into schools based on socioeconomic status or ability. To estimate the causal effect of SVA on long-run outcomes, our main identification strategy exploits variation in the average SVA of the primary schools students attend due to school switches. While switching need not

be exogenous, conditional on first school attended, most switching behavior seems to be motivated by forces orthogonal to learning and school quality. Indeed, on average, switches generate virtually no change in school value-added (see Appendix Table A2 for more details on switches). Rather, rural Pakistani households have a large number of children,[8] and parents appear to prioritize keeping children's schools close together even if they do not attend the same school. Of 727 school switches we observe in the data, 80% occur at the same time as a sibling switching schools, starting school, or completing school. This high rate of sibling concurrent moves is not simply because switching is very frequent; a student who doesn't switch herself only experiences a sibling event in the same year 30% of the time. Nor is this because parents are dissatisfied with a school and switching siblings together; only 34% of sibling-involved switches involve switching to and from the same school at the same time. Rather, in the majority of sibling-involved switches (57%), a sibling is starting kindergarten, grade 1, moving to secondary school, or graduating secondary school.

While the low rate of siblings switching together across schools may appear puzzling at first, this is likely in part because the public sector is nominally sex-segregated and in part because parents try to keep siblings in primary and secondary school close together. As a result, parents may enroll siblings in nearby schools even if they attend different schools or have one child briefly attend a cross-sex school in which she is officially not allowed to enroll when a younger sibling starts school or until an older sibling graduates. Indeed, a sizable fraction of switches (33%) involve switching into or out of a cross-sex public school. Consistent with this, approximately one-half of all switches occur between public schools. Slightly more than 10% of switches are also associated with a school entry or exit event, in line with substantial churn in the private sector.[9] While switching does not lead students to attend systematically better schools, as seen in Appendix Figure A4, switches do generate substantial variation in SVA.

Our main estimating equation is

$$y_{iT} = \gamma_{s_i^0} + \beta \frac{1}{N_i^Y} \sum_{t \in [2003,2007]} \widehat{SVA}_{st} + \Lambda \mathbf{X_i} + \upsilon_{iT}, \tag{2}$$

where T denotes the long-run round of interest (2011 or 2018), s_i^0 denotes the first (pre-switch) school a child is observed attending such that $\gamma_{s_i^0}$ is a fixed effect for the first school the child attends, and N_i^Y is the number of years the child is observed, such that $\frac{1}{N_i^Y} \sum_{t \in [2003,2007]} \widehat{SVA}$ is the average of the school-specific empirical Bayes SVA estimates a child is exposed to over the course of primary school. The vector of controls $\mathbf{X_i}$ always

[8] During our study period, the total fertility rate was approximately 4.5.

[9] Between 2003 and 2007, 47 schools enter and 52 schools exit.

includes age and gender fixed effects and variously also includes socioeconomic controls (indicator variables for mother and father have some education, whether the household head is high caste, and an index of household assets calculated in the first round the household is observed). In regressions to test for alternative stories and mechanisms, $\mathbf{X_i}$ may also include endogenous controls to account for different mechanisms (measures of average peer quality, share of years observed in a private school, and measures of whether last school attended offers higher levels of education). Throughout, we cluster at the household-level to account for within-household correlations in outcomes and school choices. In addition, for robustness, we report results focusing separately on switches to better and worse schools to account for the possibility that switches to better schools may be more selected (e.g., if a specific type of parents experiment to identify a better school for their child).

Our key estimate is β, the effect of attending a higher value-added primary school on long-run outcomes. Our identification relies on the idea that controlling for the first school a child attends accounts for the sorting of children across schools due to differences in socioeconomic status or ability. Conditional on this control, any switching between schools is for idiosyncratic reasons that are unrelated to students' learning trajectories or long-term outcomes. That is, the first school control plays a similar role to the control for lagged test scores in the conventional value-added estimation equation (see equation (1)). The alternative would be to control for baseline test scores, following the standard logic of value-added estimation, but this is not possible because, as described in Section 2.2, most of the children we observe in our long-run sample were not in the cohorts that were tested in 2003–2007 and hence, we do not have test score data for these individuals.

Our identification assumption could be violated for several reasons. First, better students (conditional on first school attended) in levels could switch into better schools. Second, students who are improving faster (conditional on first school attended) could switch into better schools. Finally, students who switch into better schools could be differentially likely to attrit from the sample. We test for each of these potential threats in turn.

Differential Switching & Student Characteristics In Appendix Table A3, we regress student characteristics on an indicator variable for whether they were observed switching primary schools controlling for first school fixed effects. Conditional on first school fixed effects, there is no directionally systematic or statistically significant relationship between switching and socioeconomic status or academic achievement. An F-test that jointly tests whether all of the covariates are related to switching cannot reject a null relationship ($p = 0.921$). Switchers do not appear to differ systematically from non-switchers.

In Appendix Table A4, we restrict the sample to switchers and evaluate whether wealthier

or higher achieving students switch into higher SVA schools conditional on first school fixed effects. There is again no evidence of a relationship between child characteristics and the quality of the school into which they switch. With the exception of an indicator variable for fathers living elsewhere (one of the possible reasons father education would be missing), which affects 30 observations, there are no statistically significant relationships between the measures of socioeconomic status and ability and the measure of new school value-added. F-tests testing whether the covariates jointly predict the value-added of the new school again cannot reject that there is no relationship.

Differential Switching & Student Trajectories Even if students' time-invariant characteristics do not predict switches, students who are on different trajectories or who experience time-varying shocks may switch into different schools. This could occur if parents observe that a child is performing well and decide to invest more in her education (e.g., by switching her to a private school) or if economic stress leads a child to switch out of private school to a public school. To evaluate whether this is the case, we estimate the following event study regression

$$y_{icst} = \sum_{k \in \{-3,..,2\}} \tau_k event_{ist}^k \Delta \widehat{SVA}_{st} + \phi_{s_i^0 ct} + \Gamma \mathbf{X_{icst}} + \epsilon_{igst},$$

where y_{igst} is either a student i's mean test scores in year t, k denotes event time (the years around a school switch), and c denotes the student's cohort (3rd grade in year 1 or 3). The indicator variable $event_{ist}^k$ is equal to 1 if a student is observed k years after an event, $\Delta \widehat{SVA}_{st}$ is the change in the estimated SVA at the time of the switch, $\phi_{s_i^0 ct}$ is a fixed effect at the first school-cohort-year level, and the vector $\mathbf{X_{icst}}$ includes controls for age and gender fixed effects.[10]

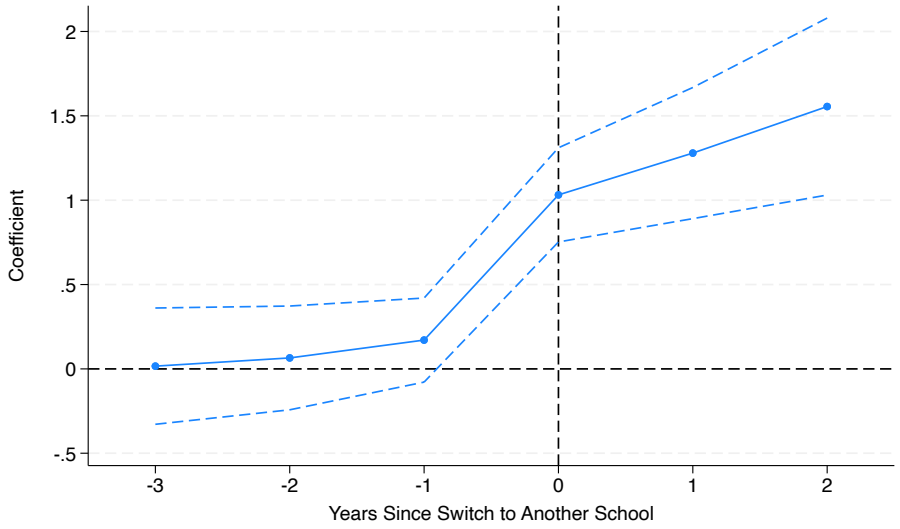
Figure 1 plots τ_k. We note that for this test, school value-addeds are measured in test score standard deviations to allow for easy assessment of forecast unbiasedness, while for our main results, the value-added measures are standardized in school value-added standard deviations. To improve power, we include all test-takers rather than just students for whom

[10]To maximize power, we use the full set of test score data collected in schools to estimate the test score event study graph, rather than restricting to children we observe in the long-run data. Controlling for the interaction between first school and year is important for this event study graph because mechanically, to be observed switching, a student must be observed attending school for longer and she will therefore accumulate larger test score gains. For similar reasons, because the data were collected from two cohorts who mechanically will be more likely to switch at different times and have more or less education, we must also interact this with cohort fixed effects. Not doing so would mechanically lead to pre-trends even in a case with zero selection.

we have long-run outcomes. The figure shows that there is no pre-trend in test scores prior to a school switch. After the switch, the school's value-added predicts test score gains with a coefficient of 1 in period 0, exactly as we would expect if the first school fixed effect fully accounts for selection. The positive effect of attending a better school accumulates thereafter, consistent with the idea that greater exposure increases learning gains.[11] Altogether, switches into better schools are not driven by students who are learning faster.

In Appendix Figure A5, we again plot the event study graphs for test scores using only switches to better schools (Panel (a)) or worse schools (Panel (b)), motivated by the intuition that switches to better schools may exhibit more selection. Even when we exclusively focus on switches to better schools, there is no evidence of a positive pre-trend, and the coefficient is close to 1 in the year of the switch, consistent with forecast unbiasedness. The graph for switches to worse schools (Panel (b)) also shows little evidence of a pre-trend and forecast unbiasedness in the year of the switch.

Figure 1: Event Study Graph for Mean Test Scores for School Switchers



Notes: Event study graph of mean test scores regressed on indicator variables for years prior to and after a school switch interacted with the change in mean empirical Bayes school value-added (SVA) between the child's new and old school. The regression includes age, gender, and cohort-by-first primary school-by year fixed effects. Standard errors are clustered at the individual level. 95 percent level confidence intervals are displayed.

[11] This differs from what one would expect if we controlled for lagged test scores to account for selection, as is standard in the value-added literature. In that case, we would estimate annual increases rather than effects accumulated across multiple years and would expect a coefficient of 1 in all post-periods, not just period 0.

Differential Attrition Finally, we evaluate whether our results could be confounded by differential attrition from the 2018, long-run follow-up survey. In Appendix Table A5, we include all participants in the sample frame and regress an indicator variable for being surveyed at all (either in-person or over the phone) and an indicator for being surveyed in-person on the measure of average school quality, conditioning on the first school fixed effect. Given that individuals are surveyed 15 years after the first round of data collection, the response rate is high (86% with 77% in-person). Furthermore, primary school quality has no effect on the likelihood an individual is surveyed (or surveyed in-person).

3.3 Movers Strategy

In addition to our primary switchers empirical strategy, we estimate a specification closely following the movers strategy in Chetty and Hendren (2018). As in Chetty and Hendren (2018), the specification only compares switchers and exploits variation in the timing of the switch — and therefore, variation in how long a child is exposed to the new school — rather than whether not a switch occurred. We restrict the sample to children who switch schools and estimate

$$y_{iT} = \kappa \widehat{SVA}_{i,s_i^0} + \sum_{j=1}^{3} \theta_j Avg\ Post\ Switch\ SVA \times I_i(N_i^{s^1} = j) + \Lambda \mathbf{X_i} + \eta_{iT},$$

where $\widehat{SVA}_{i,s^0}$ is the estimated SVA of the first school i attended, $Avg\ Post\ Switch\ SVA$ is the average estimated SVA of the schools the child attended post-switch, and $I_i(N^{s_i^1} = j)$ is an indicator variable equal to 1 if the child is observed for j years in primary school post-switch. The vector of controls $\mathbf{X_i}$ here always controls for gender, age fixed effects, and age at time of switch fixed effects. If this strategy is valid, we expect that $\theta_1 < \theta_2 < \theta_3$ since the quality of the post-switch school should matter more for a child's outcomes the longer his or her exposure. In addition, if both this strategy and the previous switchers strategy are valid, we should expect θ_3 to be similar in magnitude to β (the estimate from our main empirical strategy). This is because β should be interpreted as the effect of attending a 1 sd better school for every year we observe a child (an average of 2.6 years), while θ_3 is the effect of being observed in a 1 sd better school for 3 years. Following Chetty and Hendren (2018), we also evaluate robustness to controls for observed changes in assets at the time of the switch, in case switches are associated with income shocks, and focusing on switches that are plausibly more exogenous.

3.4 Lasso 2SLS

We complement the switcher identification strategy with a second strategy that exploits cross-sectional variation in access to higher quality schools as opposed to longitudinal variation in the schools children attended. We exploit the fact that school choice is extremely distance sensitive (Bau, 2022; Carneiro et al., 2024). Hence, children who are closer to good schools will be more likely to attend good schools all things equal. While school locations are not exogenous — in particular, we would expect private schools (which are higher quality on average) to locate closer to richer households — Andrabi et al. (2025) argue that controlling for distance to the center of the village accounts for the socioeconomic variation in household locations. This argument relies on the fact that the villages we study were originally developed as planned communities by British colonialists, with richer landowners placed in the middle of the village and poorer workers living on the outskirts. The identification therefore relies on comparing two households that are equally distant from the center but where one happens (for example) to be on the same side of the village as a low quality school, while the other is relatively farther from that school.

To maximize power and choose distance instruments in a principled way, we follow the lasso IV strategy of Belloni et al. (2012) and use a lasso regression to select the instruments that are most predictive of $\frac{1}{n^t}\sum_{t\in[2003,2007]}\widehat{SVA}_{st}$. For our pool of potential instruments, we calculate the average SVA of schools within 500 meters of a student, 600 meters, 700 meters, and so on up to 2 km. Since some students do not have any schools within a given radii, we also forceably include controls for missing values for the potential instruments and code missings as zeros. The new second stage is identical to equation (2) except that it includes controls for village instead of first school fixed effects, age by gender fixed effects, and distance to the village center and its interaction with gender. This choice of controls adheres to the controls in the related distance IV specifications used to estimate the contemporaneous private school premium for test scores in Andrabi et al. (2025).

In addition to reporting the lasso IV results results, we use the same instruments chosen by the lasso regression and report results from a two-sample IV strategy (Inoue and Solon, 2010). The two-sample IV allows us to use a larger sample (all children for whom we have distance data and school choice data regardless of whether they appear in the long-term data) to estimate the first stage, increasing power.

4 Long-Run Effects on Educational Attainment

4.1 Results from Switchers Strategy

Years of Education We begin by reporting the long-run effects of attending a higher quality school on years of education. As this is our primary outcome of interest, we report estimates with and without controls for the first school fixed effect to better evaluate the sensitivity of the results. In addition, we report the coefficients for our control variables as well as our main explanatory variable of interest. Table 1 reports the results. Our preferred specification with first school fixed effects appears in column 3, with its binscatter corollary appearing in Appendix Figure A6. We estimate that attending a 1 standard deviation better school increases years of education by 1 year. In line with the tests in the previous section, there is little evidence of bias conditional on the inclusion of the first school fixed effect. Including controls for socioeconomic status and parental education has little effect on the coefficient of interest but increases the adjusted R^2 by 0.1, suggesting that the controls are meaningful predictors of student outcomes. In contrast, columns 1 and 2 show that the estimates are more sensitive to the inclusion of controls when we do not condition on a first school fixed effect. Finally, Appendix Figure A7 reports estimates of the heterogeneous effects of school quality by measures of household socioeconomic status. The effects are generally stable across different measures of socioeconomic status, suggesting that school quality neither exacerbates or closes existing inequalities.

Effects by Level of Schooling To understand when in the life cycle the gains in educational attainment occur, in Table 2, we estimate the effects of attending a better school on each level of schooling. For every level, the effects are positive, though they become larger and more statistically significant for outcomes later in a student's educational trajectory. Attending a 1 sd better school increases completing 8th grade by a statistically insignificant 7 ppt (10%), completing 9th and 10th grade by a significant 14 ppt (29% and 34%), completing a high school diploma by a significant 10 ppt (51%), and attending college by a significant 6 ppt (48%). The point estimates are largely insensitive to the inclusion of the socioeconomic controls. Notably, the significant effect on educational attainment appears before students take their first high stakes exam, the matriculation exam at the end of 10th grade. Indeed, the effects on 9th and 10th grade completion are almost identical. Hence, the effects of school quality one educational attainment are not entirely driven by an increase in the likelihood of passing high-stakes hurdle examinations.

Table 1: The Effect of School Quality on Years of Education

	Village FE		First School FE	
	(1) Baseline	(2) + SES	(3) Baseline	(4) + SES
Empirical Bayes SVA	0.824***	0.567***	0.987***	0.867***
	(0.097)	(0.085)	(0.283)	(0.256)
Female	0.009	-0.104	0.381	0.400*
	(0.137)	(0.130)	(0.243)	(0.240)
Assets in 2003		0.342***		0.337***
		(0.042)		(0.046)
High Caste		0.539***		0.579***
		(0.181)		(0.193)
Mom Educ.		1.407***		1.373***
		(0.183)		(0.194)
Dad Educ.		1.439***		1.303***
		(0.167)		(0.184)
Mean Outcome	8.282	8.282	8.299	8.299
Adjusted R^2	0.136	0.258	0.200	0.296
Observations	3165	3165	3092	3092
Clusters	1363	1363	1332	1332

Notes: The dependent variable is the number of years of education completed. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007, and SVA is standardized by the school value-added distribution. All columns include age fixed effects and a control for gender. Socioeconomic status controls in even columns include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Controls for missing values are also included. Columns 1–2 include village fixed effects, and columns 3–4 include a fixed effect of the first primary school the individual attended. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: The Effect of School Quality on Each Level of Schooling

	Completed Grade 8		Completed Grade 9		Completed Grade 10		Completed Grade 11		High School Diploma		Went to College	
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES	(7) First Sch. FE	(8) + SES	(9) First Sch. FE	(10) + SES	(11) First Sch. FE	(12) + SES
Empirical Bayes SVA	0.069	0.059	0.135***	0.121***	0.143***	0.131***	0.116***	0.107***	0.099***	0.091***	0.054**	0.049**
	(0.047)	(0.044)	(0.041)	(0.038)	(0.039)	(0.035)	(0.033)	(0.031)	(0.031)	(0.030)	(0.022)	(0.022)
Mean Outcome	0.660	0.660	0.468	0.468	0.428	0.428	0.221	0.221	0.191	0.191	0.115	0.115
Adjusted R^2	0.159	0.223	0.170	0.244	0.185	0.257	0.143	0.206	0.117	0.173	0.109	0.150
Observations	3092	3092	3092	3092	3092	3092	3092	3092	3092	3092	3092	3092
Clusters	1332	1332	1332	1332	1332	1332	1332	1332	1332	1332	1332	1332

Notes: The dependent variables are indicator variables that equal 1 if the individual completed grade 8 (columns 1–2), grade 9 (columns 3–4), grade 10 (columns 5–6), grade 11 (columns 7–8), high school (columns 9–10), and if they attended any postsecondary institution (columns 11–12). *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007, and SVA is standardized by the school value-added distribution. All columns include age fixed effects, a control for gender, and a fixed effect for the first primary school the individual attended. Socioeconomic status controls in even columns include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index of households assets (measured in 2003). Controls for missing values are also included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Cognitive and Non-Cognitive Skills Motivated by a literature that suggests that the effects of educational interventions on cognitive skills quickly fadeout but may re-emerge through non-cognitive skills later on, we report SVA's effects on cognitive and non-cognitive skills in Table 3. Columns 1–2 report effects on mean test scores (across math, Urdu, and English) the last year a student was observed between 2003 and 2007, columns 3–4 report the effects in 2011 (8 years after the first round of LEAPS), while columns 5–6 report effects on mean test scores in 2018 (15 years later). We note that the samples for whom we have test scores in 2003-2007, 2011, and 2018 somewhat differ. This is both because only specific cohorts were originally tested between 2003 and 2007 and because the household tracking in the 2011 round missed children who were no longer in their original household. Columns 7–8 report the results using life skills measures, which test literacy and numeracy with questions that mimic how those skills are used in day-to-day life. Columns 9–10 report effects on a summary measure of non-cognitive skills, which is the first principal component from questions related to "Big Five" personality measures, a grit measure, and two tablet tasks that measure risk-taking and self control. There are large effects of school quality on test scores between 2003-2007, consistent with the valid estimation of SVA, which somewhat fade out over time. The point estimates for the effects on cognitive skills are similarly-sized in 2011 and 2018-19, though less precise in 2011, with no evidence of fadeout over the subsequent 7 years. Being exposed to 1 sd higher SVA school increases cognitive skills measures by 0.15-0.18 sd in 2018-2019. For comparison, a 1 sd better school increases test scores contemporaneously by 0.27 sd.

We observe similar effects on the life skills measures of numeracy and literacy. In contrast to Banerjee et al. (2025), who suggest there may be limited transferability between math and reading skills in the classroom and everyday life, we find a large and significant increase in life skills measures of 0.3 sd. However, we find no evidence that attending a school that increases test scores more has any impact on non-cognitive skills.

Table 3: The Effect of School Quality on Skills

	Cognitive Skills (2003-07)		Cognitive Skills (2011-12)		Cognitive Skills (2018-19)		Life Skills Index		Non-cognitive Skills	
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES	(7) First Sch. FE	(8) + SES	(9) First Sch. FE	(10) + SES
Empirical Bayes SVA	0.561***	0.592***	0.120	0.079	0.176*	0.147*	0.287***	0.267***	0.016	0.016
	(0.134)	(0.141)	(0.090)	(0.090)	(0.091)	(0.085)	(0.083)	(0.082)	(0.082)	(0.083)
Mean Outcome	0.347	0.347	0.784	0.784	0.170	0.170	0.176	0.176	0.027	0.027
Adjusted R^2	0.300	0.312	0.208	0.249	0.205	0.279	0.131	0.194	0.180	0.178
Observations	1121	1121	2001	2001	2763	2763	2765	2765	2765	2765
Clusters	900	900	1162	1162	1274	1274	1275	1275	1275	1275

Notes: The dependent variable in columns 1 to 6 is the average score from a test in math, Urdu, and English, measured in the last year observed between 2003-07 (columns 1–2), 2011-12 (columns 3–4) and in 2018-19 (columns 5–6). In columns 7 and 8, the outcome is a score measuring literacy and numeracy. In columns 9 and 10, the outcome is an index from Danon et al. (2024) measuring socioemotional skills. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007, and SVA is standardized by the school value-added distribution. All columns include age fixed effects, a control for gender, and a fixed effect of the first primary school the individual attended. Socioeconomic status controls in even columns include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Controls for missing values are also included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Robustness of Switchers Strategy

In this subsection, we provide additional evidence that the estimates generated by the switcher identification strategy are unbiased. For our first test, we separately estimate the effects of average school value-added for students who switch to better vs. worse schools. Intuitively, if there is selection, we would expect that it would be more likely to occur for "good" switches, so it is reassuring if estimates from "bad" switches are similar. Our latter two tests are inspired by tests from Chetty and Hendren (2018). First, we assess the robustness of our results to keeping the switches that appear the least likely to be contaminated by selection and dropping all other switches. Second, we separately estimate school value-added by type (gender or socioeconomic status). If the effects we estimate are causal, school value-added for own type should be a better predictor of an individual's long-run outcomes than value-added for the other type. On the other hand, selection would only lead to this pattern if households' enrollment decisions respond not just to school value-added but also type-specific school value-added, requiring quite a bit of information and sophistication on the part of households.

Effects for Switches to Better and Worse Schools Appendix Table A6 re-estimates the specifications in columns 3–4 of Table 1, interacting the average SVA measure with indicator variables for whether a student switched into a worse school, a better school, or no switch occurred. We find that school quality has similarly-sized, large positive effects for all three types of students. The point estimates for bad switches closely replicate the main estimates in Table 1.

Focusing on Most Plausibly Exogenous Switches In Appendix Table A7, we show that the results are robust to focusing on the most plausibly exogenous subsets of switches. In columns 3–4, we restrict the sample to non-switchers and students who switch schools concurrently with a sibling starting school, finishing school, or switching schools or because their school exits. We further drop switches where the siblings either start in or end in the same school to eliminate cases where positively selected parents are trying to move both to a school that is perceived as better or take both out of a school that is perceived as failing. The resulting point estimates are similarly-sized (though slightly smaller) than those in Table 1 and more imprecise, consistent with the reduced sample size. In columns 5–6, we instead restrict the set of switchers to those whose switch is associated with school closure or who have a sibling starting kashi (the equivalent of kindergarten), 1st grade, middle school, or lower or upper secondary school (providing an exogenous reason for a switch). The point estimates are again similar to (though slightly larger than) the baseline point estimates

(columns 1–2).

Predictive Power of Type-Specific School Value-Addeds Finally, we re-estimate the school value-added measures separately by gender and socioeconomic status (asset index is above or below the sample median). We then re-estimate the specifications from Table 1 with the SVA for an individual's own type and the other type as the explanatory variables. We note that for gender this greatly reduces sample size since we can only include individuals in schools where SVA can be estimated for both boys and girls. In cases where sex-segregation is strictly enforced, this will not be possible. Appendix Table A8 reports the results. To allow for a comparison of the point estimates with consistent samples, columns 1–4 restrict to the sample that attended schools for whom both male and female-specific SVAs could be calculated. Columns 5–8 restrict to schools for whom both high and low socioeconomic status SVAs could be calculated. Reassuringly, own gender SVA and own SES SVA have larger effects on a student's outcomes than the opposite type's SVA, consistent with school quality causally affecting students' educational attainment.[12]

4.3 Results from Alternative Empirical Strategies

Movers Strategy Table 4 reports the results from the movers strategy where we restrict the sample to students that switch schools. Columns 1 and 2 confirm that switching into a better school strongly increases years of education, while columns 3 and 4 show that the effects of switching into a better school increase monotonically with the amount of time spent in the new school. Reassuringly, the effect of spending 3 years in a 1 sd better school is quantitatively similar to the effect of attending a 1 sd better school on average in Table 1. Following Chetty and Hendren (2018), we show that the estimates (including the pattern of increasing effects of school quality with more time spent in the school) are robust to controlling for observed changes in assets at the time of the switch to account for socioeconomic shocks (columns 5–6). In columns 7–10, we again focus on the most plausibly exogenous switches. Because this greatly reduces sample size, to improve precision, we replicate the specifications in columns 1–2 rather than separately estimating the effects of school quality by duration. While the point estimates are less precise, if anything, they are

[12]If there were no measurement error in the SVAs, we would expect the opposite type's SVA to have no predictive power, as is the case for the gender regressions. However, SVAs are likely measured with error since we only observe finite samples of students of a given type in a school. Measurement error can lead both explanatory variables to have predictive power, as in column 8, if they both noisily measure a common effect across students along with a type-specific effect. The increase in the point estimate for the opposite type SVA in column 8 when own-type SVA is included as a control is consistent with the well-known suppressor effect in statistics.

larger when we focus on this set of switchers.

Table 4: Movers' Estimates of Effect of School Quality on Years of Education

	Baseline		× Exposure		Age of Switch × δ Assets		Excl. Switchers Moving w/ Sibling		Switchers with Sib. Starting Sch. Level	
	(1) Baseline	(2) + SES	(3) Baseline	(4) + SES	(5) Baseline	(6) + SES	(7) Baseline	(8) + SES	(9) Baseline	(10) + SES
Post-switch Emp. Bayes SVA	0.642***	0.525**					0.839*	0.828*	0.834**	0.707*
	(0.228)	(0.205)					(0.448)	(0.420)	(0.401)	(0.361)
Post-switch Emp. Bayes SVA × One Year			0.536*	0.405	0.538*	0.375				
			(0.280)	(0.246)	(0.288)	(0.252)				
Post-switch Emp. Bayes SVA × Two Years			0.688**	0.600**	0.645**	0.534*				
			(0.275)	(0.261)	(0.295)	(0.276)				
Post-switch Emp. Bayes SVA × Three Years			1.011**	0.833**	1.032**	0.864**				
			(0.452)	(0.419)	(0.452)	(0.420)				
Mean Outcome	8.390	8.390	8.390	8.390	8.390	8.390	8.330	8.330	8.527	8.527
Adjusted R^2	0.152	0.260	0.147	0.259	0.142	0.260	0.176	0.313	0.215	0.345
Observations	554	554	554	554	554	554	179	179	237	237
Clusters	394	394	394	394	394	394	148	148	157	157

Notes: The dependent variable is the number of years of education completed. *Post-switch Emp. Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended after a switch. Baseline controls include the mean empirical Bayes school value-added of the first school the student attended, a control for gender, and fixed effects for age, age at the time of the switch, and village. Socioeconomic status (SES) controls in even columns include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Controls for missing values are also included. Columns 3–6 interact *Post-switch Emp. Bayes SVA* with indicator variables for the number of years we observe the individual enrolled in a primary school after the switch and include number of years observed in primary school post-switch fixed effects. Columns 5–6 also include controls for age at the time of the switch fixed effects interacted with a measure of the change in assets the household experienced the year of the switch. Columns 7–10 are similar to 1–2, but 7–8 restrict the sample to switchers whose switch involved a school closure or a sibling entering, dropping, or switching school, excluding switchers if their sibling ever switched to the same school. Columns 9–10 restrict the sample to switchers whose switches involved a school closure or a sibling starting or switching school in kashi, grade 1, middle school, lower or upper secondary school. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Lasso-IV Strategy Finally, we evaluate whether an alternative identification strategy that leverages cross-sectional variation in students' locations rather than longitudinal variation in the schools they attend produces similar results. As described in Section 3, we use lasso to select the instruments that are most predictive of the average SVA of the schools a child attends from a pool of measures of the average SVA of schools within different radii of the child, conditional on distance to the center of the village. The first column of Table 5 reports the selected instrument and the F-statistic of the first stage. Appendix Table A9 reports the associations between the instruments and different measures of household socioeconomic status, conditional on distance to the center of the village. Each column reports a F-statistic which tests if the instruments are jointly related to a given covariate. The joint tests show that there is no relationship between the selected instruments and the measures of household socioeconomic status, supporting the exclusion restriction. Column 2 of Table 5 reports the estimated effect. While underpowered relative to the switcher identification strategy, the results are qualitatively similar. Here, attending a 1 sd deviation better school increases years of education by 1.4 years. While we note that the local average treatment effect for this strategy could differ from the average treatment effect estimated with the switchers strategy, we do observe that our switcher estimate is within the confidence interval of the distance IV strategy. In columns 3 and 4, we re-estimate the IV strategy using a two-sample IV strategy (Inoue and Solon, 2010). The two-sample IV strategy allows us to use all children for whom we have distance and school quality information in the first stage, even if we do not observe their long-run outcomes. The effect of average school quality is now more precisely estimated. Otherwise, the results are broadly similar (with an estimated effect of 1.7 years of education). Reassuringly, a secondary strategy using orthogonal variation to our main strategy arrives at similar results.

5 Channels

While we have focused on the fact that high SVA schools directly improve foundational learning as the main explanation for our findings, there are other possibilities. Below, we explore the role of these potential explanations.

Reputation & Peer Effects It could be that high SVA schools also have better reputations and that this ultimately improves students' ability to gain admission to and attend better secondary schools and colleges (MacLeod and Urquiola, 2015; Sekhri, 2020). Alternatively, it could be that schools that have high SVA increase students' outcomes because they provide students with better peers and better peer effects rather than that cognitive skills

Table 5: IV Estimates of the Effect of School Quality on Years of Education

	Lasso IV		TS2SLS Lasso IV	
	(1)	(2)	(3)	(4)
	First Stage	Years of Education	First Stage	Years of Education
Avg. SVA within 1.2 km	0.170***		0.165**	
	(0.063)		(0.067)	
Avg. SVA within 1.7 km	0.406***		0.298***	
	(0.107)		(0.101)	
Empirical Bayes SVA		1.411		1.762***
		(0.895)		(0.586)
F-statistic	13.460		11.550	
Observations	3165	3165	4580	
Observations 1st Stage				4580
Observations 2nd Stage				3143

Notes: Columns 1–2 show the first and second stages of the instrumental variable (IV) strategy, and columns 3–4 use a two-sample IV strategy following Inoue and Solon (2010). *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. The potential instruments included are the average SVA of schools in different radii ranging between 500 meters and 2 kilometers in increments of 100 meters. The instruments are selected using the lasso IV strategy. Missing indicator variables are included for all potential instruments. All regressions include controls for village fixed effects, age-by-gender fixed effects, distance to the village center, and its interaction with gender. Standard errors are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

improve long-run outcomes. To rule out both these possibilities, in Table 6, we compare our baseline estimates (column 1) to the estimated effects when we include rich controls for peer quality, which capture both direct peer effects and the reputational component of school quality. While one of the peer quality measures – share of fathers with at least some education – does positively predict years of schooling, the inclusion of the peer controls has almost no effect on our coefficient of interest. Appendix Figures A8 and A9 report the results with the additional peer quality controls for other outcomes and show that the results are largely insensitive to their inclusion.

Private Premium Another possibility is that SVA is correlated with private schooling (as shown by Andrabi et al. (2025)), and our estimates capture some kind of private school premium for educational attainment rather than the direct effects of attending a school with a larger effect on cognitive skills. To evaluate whether this is the case, in column 3 of Table 6, we also control for the share of time a student spends in private school. The coefficient on this control is positive and imprecise. However, the coefficient for SVA remains similarly large and statistically significant. Appendix Figures A8 and A9 report the results with the additional private schooling controls for other outcomes and show they remain stable.

Table 6: Tests of Alternative Channels

	Dep. Var.: Years of Education			
	(1) Baseline	(2) Peer Quality	(3) Share Private	(4) Last Grade
Empirical Bayes SVA	0.987***	0.916***	0.776***	0.878***
	(0.283)	(0.264)	(0.272)	(0.263)
Female	0.381	0.393	0.401*	0.410*
	(0.243)	(0.240)	(0.240)	(0.240)
Assets in 2003		0.335***	0.336***	0.336***
		(0.046)	(0.046)	(0.046)
High Caste		0.564***	0.580***	0.561***
		(0.193)	(0.193)	(0.193)
Mom Educ.		1.371***	1.365***	1.364***
		(0.195)	(0.194)	(0.195)
Dad Educ.		1.317***	1.302***	1.312***
		(0.185)	(0.184)	(0.185)
Peers' Mom Educ.		-1.486*		
		(0.900)		
Peers' Dad Educ.		2.602***		
		(0.961)		
Peers' Assets		-0.366		
		(0.230)		
Time Share in Priv.			0.438	
			(0.471)	
Offers at least some Middle School				0.064
				(0.308)
Offers at least some Matric				0.347
				(0.342)
Offers at least some Higher Secondary				-0.184
				(0.608)
Baseline Controls	Yes	Yes	Yes	Yes
Parental Controls	No	Yes	Yes	Yes
Peers Quality	No	Yes	No	No
Private School	No	No	Yes	No
Last Grade Offered	No	No	No	Yes
Mean Outcome	8.299	8.299	8.299	8.302
Adjusted R^2	0.200	0.299	0.296	0.295
Observations	3092	3092	3092	3085
Clusters	1332	1332	1332	1329

Notes: The dependent variable is the years of education completed. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. Baseline controls include age fixed effects and a control for gender. Parental controls in columns 2 to 4 include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Controls for missing values are also included. *Peer quality* in column 2 includes three controls for the average fathers' and mothers' education and average assets of the peers in the school. Missing indicator variables are included. *Share private school* in column 3 is the share of time spent in a private primary school. *Last grade offered* in column 4 is a control for the highest grade offered by the last primary school the individual attended. We group the last grade offered into four categories: the school offers at least some primary levels (grades 1 to 5, the omitted category), middle school (grades 6 to 8), Matric (i.e., grades 9 and 10), and higher secondary (grades 11 and 12). All columns include a fixed effect for the first primary school the individual attended. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Combination Primary/Secondary Schools A third possibility is that high value-added schools are more likely to offer grades beyond primary school. Since students do not need to switch schools, this would reduce the cost of increasing educational attainment. This is a plausible alternative explanation, at least for increases in education at lower levels. About 50% of children attend schools that offer at least some middle school, though very few of the primary schools offer levels of education beyond middle school. Only 20% of students are in schools that continue through grade 10, and only 2% offer through grade 12. In the last column of Table 6, we include controls for the highest grade offered by the *last* primary school a student attended. The inclusion of these controls has little effect on our main coefficient of interest. In addition, while the coefficients on the these controls are imprecise, there is no systematic evidence that students receive more education if they attend a primary school that offers higher grades. Altogether, we conclude the effects of attending a higher SVA school appear to be driven by the direct impact on cognitive skills.

6 Employment and Wages

Our last set of results concern the effects of attending a better school on employment and wages. Unlike education, where men and women's attainment are largely similar, female and male outcomes strongly diverge when we consider employment and wages. As shown in Appendix Table A1, 7% of women report working while 76% of men do. So, when we analyze effects on labor market outcomes, we always separately report results for men and women, along with results for the pooled sample.

While these outcomes are important, we view our results here as less definitive than the educational results, which capture most of individuals' completed education. Our sampling frame consisted of those who were 5–15 in 2003-04. Given that our long-run outcomes were measured in 2018-19, this results in a sample that is roughly 17–32 when they were surveyed about labor force outcomes.[13] Appendix Figure A1 plots the percent of individuals working (Panel (a)) and enrolled in school (Panel (b)) by age in the long-term follow-up. We observe that until age 25, a fairly sizable fraction of both men and women are still enrolled in school. This appears to be important for male labor force participation, which tracks up with age as enrollment falls but has negligible effects on female labor force participation, which is always low. Thus, among men, a substantial part of our sample that is likely to enter the labor force in the future (after finishing their education) and may see the highest returns to attending a high quality primary school (as they are completing the most education) is not yet working.

[13]There are some deviations because ages are not always fully consistently reported over time.

Table 7 more directly illustrates this effect. Panel (a) shows that attending a higher quality primary school has no effect on whether an individual is engaged in a productive activity (defined as working or being enrolled in school). But this null effect masks the fact that students who attended higher SVA schools are more likely to be currently enrolled (columns 3–4) and less likely to work, albeit insignificantly (columns 5–6). These pooled effects in turn mask important heterogeneity by gender. For men (Panel (b)), where employment rates are close to universal if a man is out of school, there is a clear positive effect on enrollment and a negative and significant effect on employment, which produces an insignificant negative net effect on productive activities. Intriguingly, for women, who have a very low labor force participation rate, the results are more imprecise but positive for both working and enrollment. There is no contradiction in these two positive effects: because the female labor force participation rate is so low, enrollment does little to crowd out employment. While the estimate is only significant at a 10% level, attending a 1 sd better school over the course of the observation period increases the likelihood a woman is employed by 4.4 ppt (67%), suggesting that attending a better primary school increases female labor force participation. We speculate that this is because education gives women access to teaching jobs, one of the few socially acceptable opportunities for women in this context. In our sample, approximately 65% of women who self-report as employed report working as teachers.

In Table 8, we estimate the effect of attending a 1 sd better school on log income among those that are employed. These regressions are likely to be underestimates of the long-run effects of higher quality primary schooling for at least two reasons. First, as mentioned above, attending a higher quality school causes a man to be more likely to be enrolled in 2018, and this in turn means he is not observed working. If a large part of the benefits of attending a better school occur through increased education and/or if the students who experience increased enrollment at high levels (upper secondary school and college) are higher ability and have higher latent wages than those who have dropped out, we will underestimate the effects of attending a higher quality school. In addition, we observe students early in their careers, when those who got less education mechanically have more experience and when benefits from differential income trajectories are yet to be realized. This is consistent with Chetty et al. (2014b) in the U.S., who show that, in the case of teacher value-added, students who had a higher value-added teacher are on a steeper income trajectory, and income effects are initially negative when students are young (in their early 20s) but become positive and statistically significant at age 24. With these caveats in mind, we estimate that among working men, attending a 1 sd better school increases earnings by 11%, although this effect is imprecise and not statistically significant. For completeness, we also report results for working women. While the estimates are very large and marginally statistically significant,

Table 7: The Effects of School Quality on Labor Force Participation

	Works or Is Enrolled		Is Enrolled		Works	
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES
Panel A: Full Sample						
Empirical Bayes SVA	0.034	0.037	0.056**	0.051**	-0.021	-0.014
	(0.031)	(0.031)	(0.025)	(0.024)	(0.030)	(0.030)
Mean	0.560	0.560	0.129	0.129	0.451	0.451
Adjusted R^2	0.443	0.448	0.126	0.165	0.516	0.518
Observations	3092	3092	3092	3092	3092	3092
Clusters	1332	1332	1332	1332	1332	1332
Panel B: Men						
Empirical Bayes SVA	-0.035	-0.028	0.058*	0.057**	-0.089*	-0.079*
	(0.036)	(0.036)	(0.031)	(0.029)	(0.046)	(0.043)
Mean	0.864	0.864	0.108	0.108	0.775	0.775
Adjusted R^2	0.010	0.009	0.140	0.177	0.106	0.120
Observations	1635	1635	1635	1635	1635	1635
Clusters	1029	1029	1029	1029	1029	1029
Panel C: Women						
Empirical Bayes SVA	0.129**	0.110*	0.089	0.066	0.044*	0.044*
	(0.065)	(0.066)	(0.063)	(0.062)	(0.025)	(0.026)
Mean	0.198	0.198	0.152	0.152	0.067	0.067
Adjusted R^2	0.131	0.158	0.141	0.174	0.065	0.074
Observations	1333	1333	1333	1333	1333	1333
Clusters	829	829	829	829	829	829

Notes: The dependent variables are indicator variables that equal 1 if the individual reports working or being currently enrolled in school (columns 1–2), being enrolled in school (columns 3–4), or working (columns 5–6). Working is defined as having engaged in an activity in the past month for which the individual has received a payment. Panel A includes all individuals, Panel B is the sample of men, and Panel C is the sample of women. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. All columns include age fixed effects and a fixed effect for the first primary school the individual attended. Panel A includes a control for gender. Socioeconomic status (SES) controls in even columns include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Controls for missing values are also included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

we do not view them as reliable given the very small sample of women who work combined with the demanding data requirements of the switchers identification strategy.[14] While

[14]Measuring earnings for women is difficult because some women report not being employed but then have small, part-time earnings from tutoring or handicrafts. We did collect data on earnings in these cases

the earnings results for men are imprecise, recalling that attending a 1 sd better primary school leads to 1 additional year of education, the point estimates are strikingly close to both Mincerian estimates of the effect of an additional year of education – Patrinos and Psacharopoulos (2020) estimate the global return to a year of education is 9% – and causal estimates from Duflo (2001) and more recently, Khanna (2023). Given the close mapping between (our likely downwardly biased) estimates of the long-run returns of foundational learning on wages and the Mincerian return, in the next section, when we quantify the gains to attending better primary schools, we use estimates of the Mincerian return of a year of education as likely conservative estimates of the effect of attending a 1 sd better school.

Table 8: The Effect of School Quality on Log Income

	All		Men		Women	
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES
Empirical Bayes SVA	0.098	0.105	0.114	0.119	2.414*	1.645
	(0.085)	(0.081)	(0.083)	(0.080)	(1.286)	(1.596)
Mean Outcome	4.865	4.865	4.912	4.912	3.822	3.822
Adjusted R^2	0.141	0.150	0.054	0.060	0.539	0.718
Observations	1191	1191	1122	1122	32	32
Clusters	813	813	781	781	23	23

Notes: The dependent variable is log monthly earnings for individuals who report working. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. All columns include age fixed effects and a fixed effect for the first primary school the individual attended. Columns 1–2 include a control for gender. Socioeconomic status (SES) controls in even columns include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index of households assets (measured in 2003). Controls for missing values are also included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

7 Quantifying the Gains From Improved Learning

A policymaker in a low-income country deciding how much to invest in education and at what point in the life cycle may worry that any benefits to increasing investment in foundational

for women, but did not ask questions about earnings for men who reported being unemployed. To maintain consistency in our regressions, we do not include these women in the logged income regression. However, unsurprisingly, given the very small sample size, including all women with positive earnings leads to different point estimates ($\beta = 0.056$, se=0.980 for the baseline specification and $\beta = -0.246$, se $= 1.073$ for the specification with SES controls).

learning will fadeout by the time a student reaches the workforce 10–15 years later. In this section, we use two exercises to provide back-of-the-envelope quantifications of the benefits of better primary schooling. While any such exercise requires strong assumptions, we aim to be conservative. We assume all the wage benefits of improved learning in primary school operate through the causal effects on years of schooling, even though improved learning may have additional effects over and beyond these "Mincerian returns." Since the vast majority of women do not work, we focus only on benefits to male wages, even though attending a 1 sd better school almost doubles female labor force participation. Furthermore, we ignore all potential non-pecuniary benefits to better schooling, although there is a growing literature suggesting that education has other non-wage benefits (for example, Andrabi et al. (2012) on maternal education and children's educational outcomes in our context, Duflo et al. (2024) on the intergenerational benefits of improved schooling, or Cutler and Lleras-Muney (2006) on the link between education and health). We first consider the effects of moving children between schools. We then show how our estimates can be applied to generate cost-effectiveness estimates for any intervention that increases learning in a low-income setting.

First, to quantify the effects of better primary schooling, we estimate the present discounted wage benefits of moving all the *boys* in the worst school in the average village to the best school. Note that this will result in much lower gains than moving *all* boys to the best school in their village, as doing so would affect many more children. Combining the average gap between the worst and best school in a village estimated by Andrabi et al. (2025) with our estimates in Table 1, we estimate that moving a boy from the worst to best school in the average village would increase years of schooling by 2.06. In our data, average monthly male wages for males are 186 USD, and we assume a Mincerian return of 9%. If we assume the switch occurs when a student is in 3rd grade (in line with the fact that we typically observe students for 3 primary school years in our data) and that the student begins working at age 18, after 30 years of working, applying a discount rate of 0.95, this leads to a gain of approximately 4000 USD for each male moved from the worst to best school in the village. As the average worst primary school has 74 boys in our first round of data collection, this generates gains *for just the average village* of 302,000 USD. Even though we have moved only a small fraction of children in the village, these are large effects; Pakistan's GDP per capita in 2023 was 1,400 USD.

This suggests that there are very large benefits to students attending better primary schools. However, a policy that moves all students to the best school may not be feasible (e.g., because of capacity constraints), and a policymaker may wonder if there are also potential benefits from improving the quality of existing schools as opposed to reshuffling students. Given an intervention whose cost and short-run effects on learning are known (as

with most RCT evidence on education in low-income countries), the policymaker may ask whether the discounted labor market benefits (a conservative measure of the social benefits) of the policy outweigh the costs. To show how our estimates can be used to answer this question, we consider the effects of the median educational intervention, which Evans and Yuan (2022) estimate is 0.1 sd. This effect is often thought of as small or moderate in the literature, given that it is only a small fraction of the learning gap between poor and rich countries, but our estimates of the long-run effects of increased learning in primary school allow us to actually quantify the gains from such an intervention. Assuming the intervention increases value-added by this much for three years of primary school, this implies an increase in years of education of 0.48 years, implying additional earnings of 94 USD per year based purely on the Mincerian returns. In present discounted terms, this would increase a student's life time income by 885 USD. So, if an intervention expected to have the median effects for at least three years of primary school costs less than 885 USD per male student to implement, its pecuniary benefits would be greater than its costs.

8 Conclusion

A large literature in development and education economics has focused on how to improve foundational learning in low-income countries. Yet, high rates of fadeout of cognitive gains, even in high-income contexts where rates of school attendance are higher and students are less casually attached to school, call into question the returns on these investments. This paper is the first to our knowledge to causally link gains in foundational learning — via attending higher quality primary schools — to longer-run educational and even early-stage labor market outcomes. To do so, in a low-income context with no administrative data, we take advantage of a unique fifteen year data collection effort that includes four years of panel test score data collection in primary school — facilitating the estimation of valid school value-addeds — and tracks individuals from childhood in 2003–2007 to adulthood in 2018. These unusually rich data allow us to both link school quality with long-run outcomes and to exploit potentially exogenous variation in the schools students attend to estimate causal effects.

In contrast to concerns about fadeout, children who attend higher SVA schools at the primary-level have greater cognitive skills as adults. This may at least in part be due to the fact that children who attend higher quality primary schools receive more schooling. Attending a 1 sd better school increases educational attainment by 1 year, with gains that emerge by 9th grade and persist through secondary school completion and college enrollment. In contrast, these gains are *not* due to improved non-cognitive skills. Attending a better

primary school — and in turn, receiving more education — has no effect on a composite measure of non-cognitive skills.

Turning to the labor market, we observe a marginally significant negative effect on male labor force participation, which is fully explained by the fact that attending a better primary school increases secondary schooling and college. This in turn reduces observed employment due to an "incarceration effect" whereby men are more likely to still be enrolled. For women, whose baseline labor force participation is extremely low, this incarceration effect is limited, and attending a 1 sd better primary school has large positive effects on labor force participation. Foundational learning improves educational attainment, which in turn helps women access socially acceptable jobs as teachers in this highly gender biased setting. Our results suggest that even in settings where few jobs are available to women, education can be an important gateway for labor force participation.

While the fact that we observe young adults (and few women working) complicates estimating wage effects, our point estimates, though imprecise, are in line with Mincerian returns to education in this and other settings. This provides us with confidence in quantifying the gains to improved foundational learning under the strongly conservative assumption that all gains operate through male wages. Because our estimates allow us to link improved foundational learning with educational attainment and then earnings, we are able to more broadly characterize the earnings effects of interventions to improve learning in low-income settings. This allows for the calculation of pecuniary returns for learning interventions, which would have previously required heroic assumptions about the link between foundational learning and wages.

Altogether, our findings suggest that despite evidence of fadeout in other work, improving foundational learning has large and lasting effects, pointing to the value of investment in improving primary educational systems in low-income countries. Given that children in our context and many other low-income contexts live in settings with a high degree of choice, our results also point to the value of interventions that improve the allocation of students to higher quality schools.

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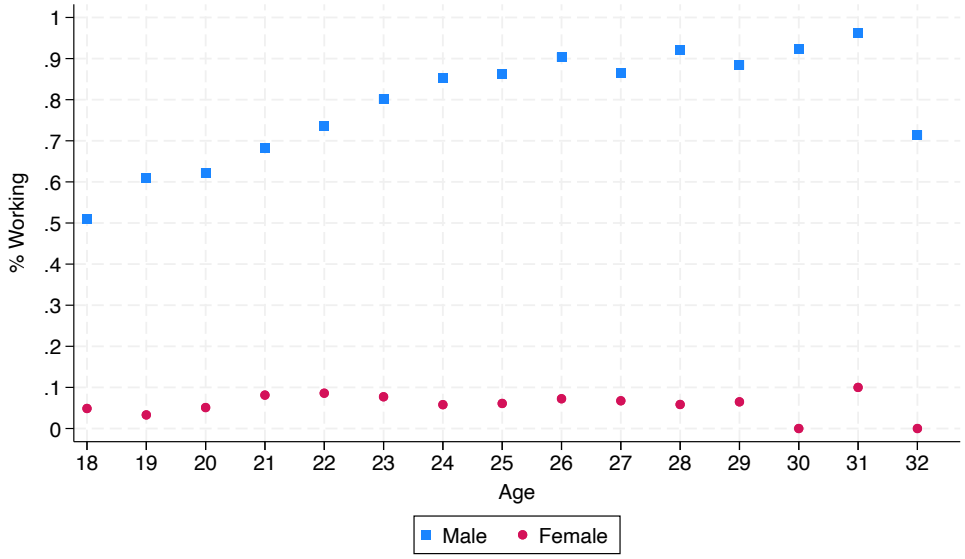
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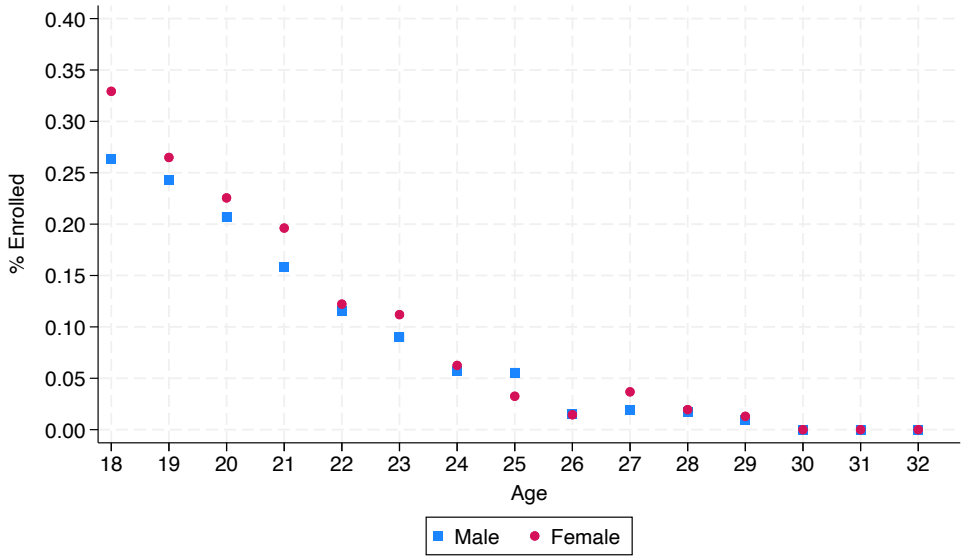
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A Appendix Figures

Figure A1: Share of Individuals Employed and Enrolled in School, by Gender and Age



(a) Share Employed



(b) Share Enrolled

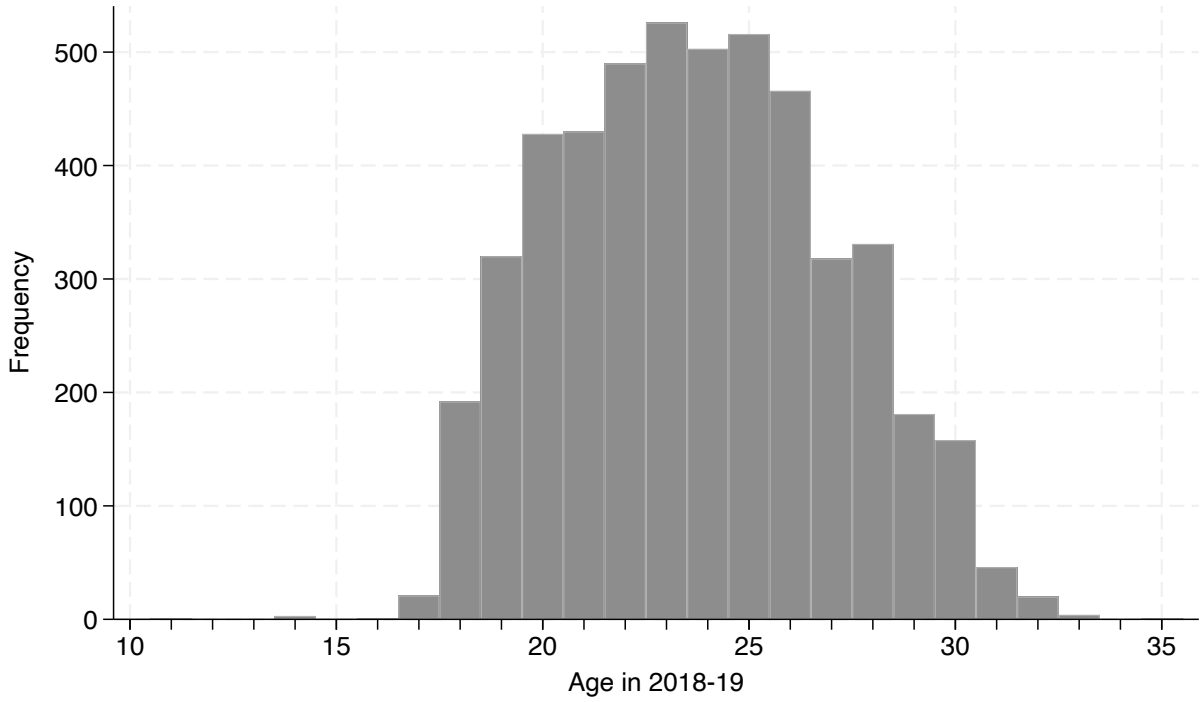
Notes: Panel (a): share of men and women of a given age who have engaged in an activity in the past month for which they have received a payment. Panel (b): share of men and women of a given age who are currently enrolled in school. The majority of individuals enrolled in school are in grade 12 (18%), or pursuing a Bachelor's (30%) or Master's degree (22%).

Figure A2: Timeline of LEAPS Data Collection

	2003-04	2004-05	2005-06	2006-07	2011-12	2018-19
Household Surveys	✓	✓	✓	✓	✓	✓
School Surveys	✓	✓	✓	✓	✓	–
Tested Cohort 1	Grade 3	Grade 4	Grade 5	Grade 6	–	–
Tested Cohort 2			Grade 3	Grade 4	–	–

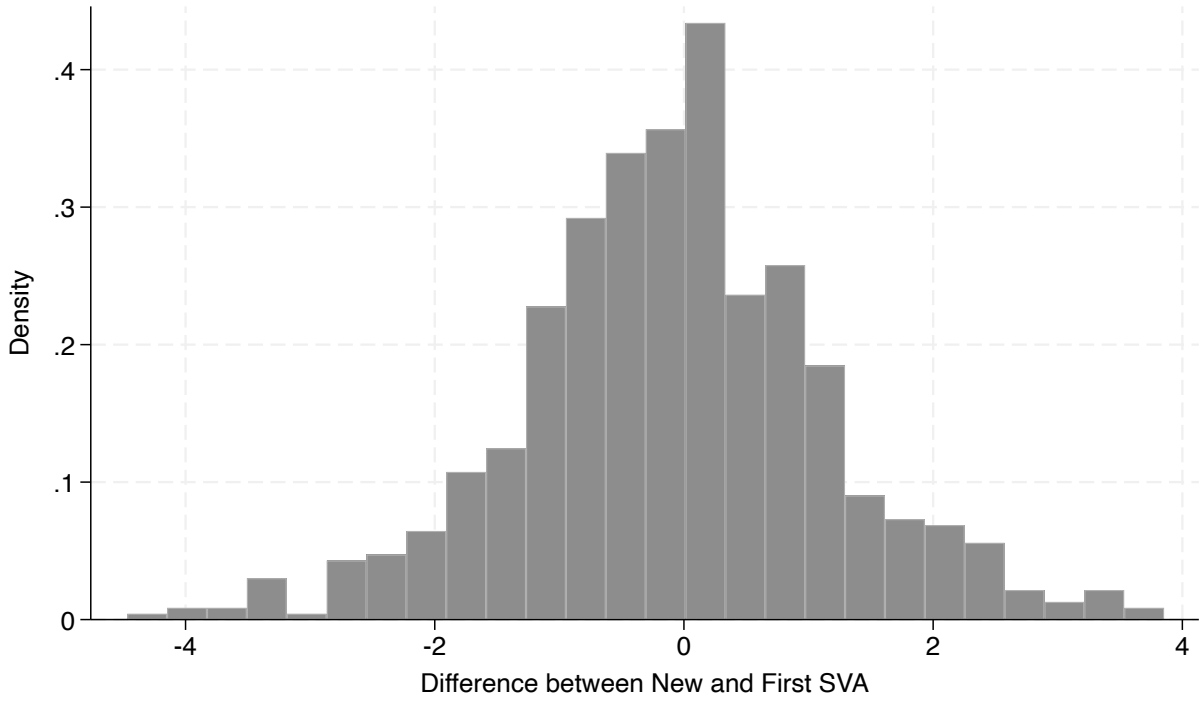
Notes: Timeline of LEAPS data collection for household and school surveys between the first round in 2003-04 and the last and sixth round of data in 2018-19. Check marks (✓) mean data were collected, and dash (−) means data were not collected. Data from the two tested cohorts are used to construct the empirical Bayes school value-added (SVA) estimates.

Figure A3: Frequency of Individuals by Age in the 2018-19 Survey



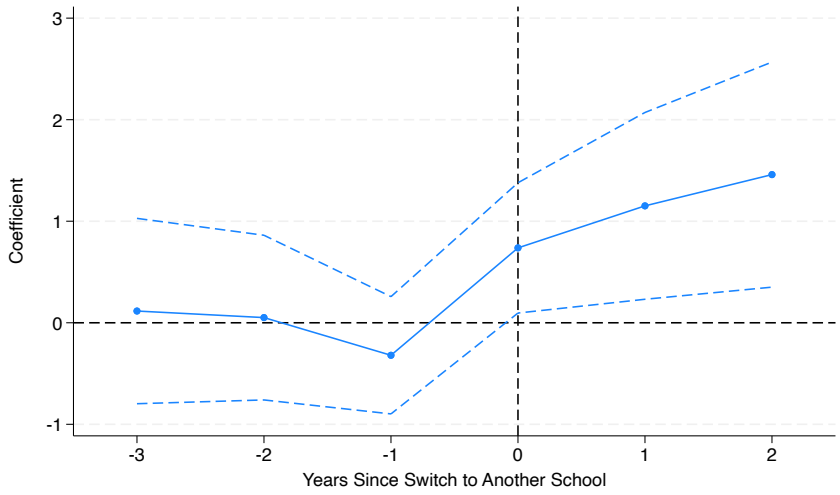
Notes: Histogram showing the frequency of individuals who completed the 2018-19 long-term follow-up survey by age. The sampling frame consisted of individuals who provided an age between 5 and 15 years old in 2003-04.

Figure A4: Histogram of SVA Differences for Switchers

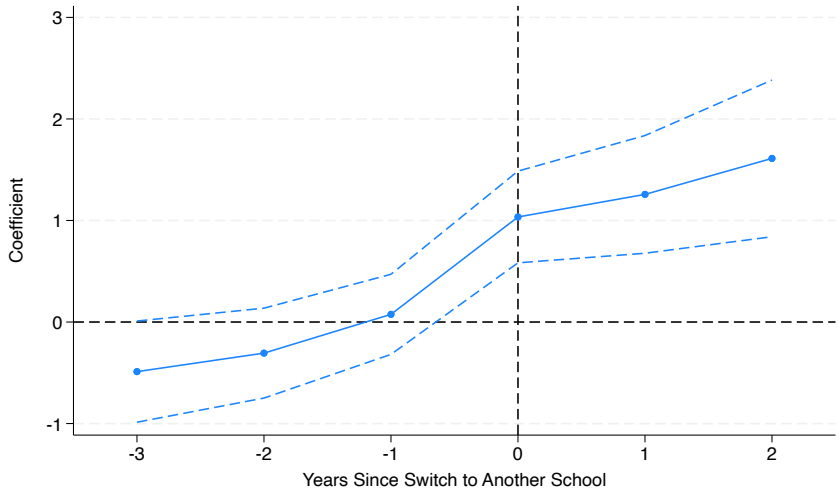


Notes: Histogram of the difference between the empirical Bayes school value-added (SVA) estimate of the new primary school switchers attend and the SVA of their previous primary school. SVA is standardized by the school value-added distribution. The sample includes 727 switches from 563 switchers.

Figure A5: Event Study Graphs for Primary School Switchers, Separating Good and Bad Switches



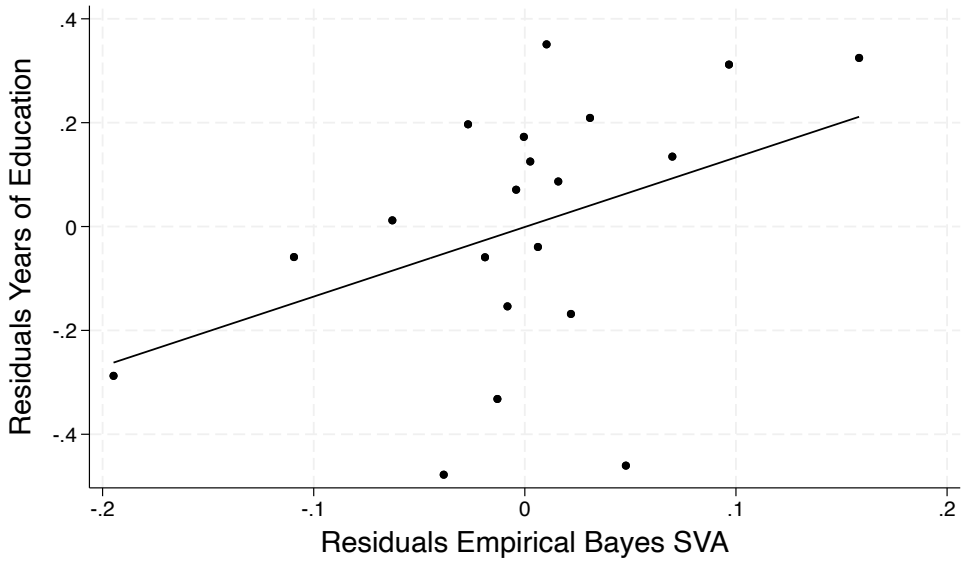
(a) Good Switches



(b) Bad Switches

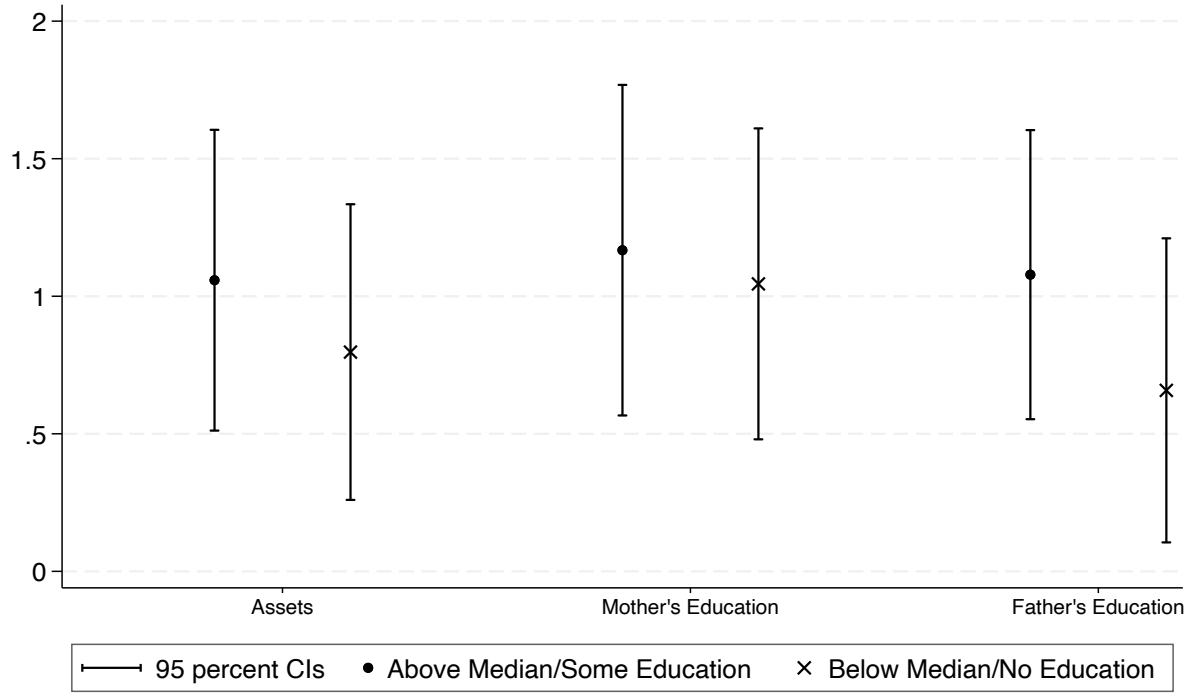
Notes: Event study graphs of mean test scores regressed on indicator variables for event time prior to and after a school switch interacted with change in mean empirical Bayes school value-added (SVA). Panel (a): sample only contains switches from a lower to higher SVA school. Panel (b): sample only contains switches from a higher to lower SVA school. All regressions include age, gender, and first school-year-cohort fixed effects. Standard errors are clustered at the child-level (since households are not observed in the child data), and the figures display 95 percent level confidence intervals.

Figure A6: Binscatter of Years of Education and Mean Empirical Bayes SVA



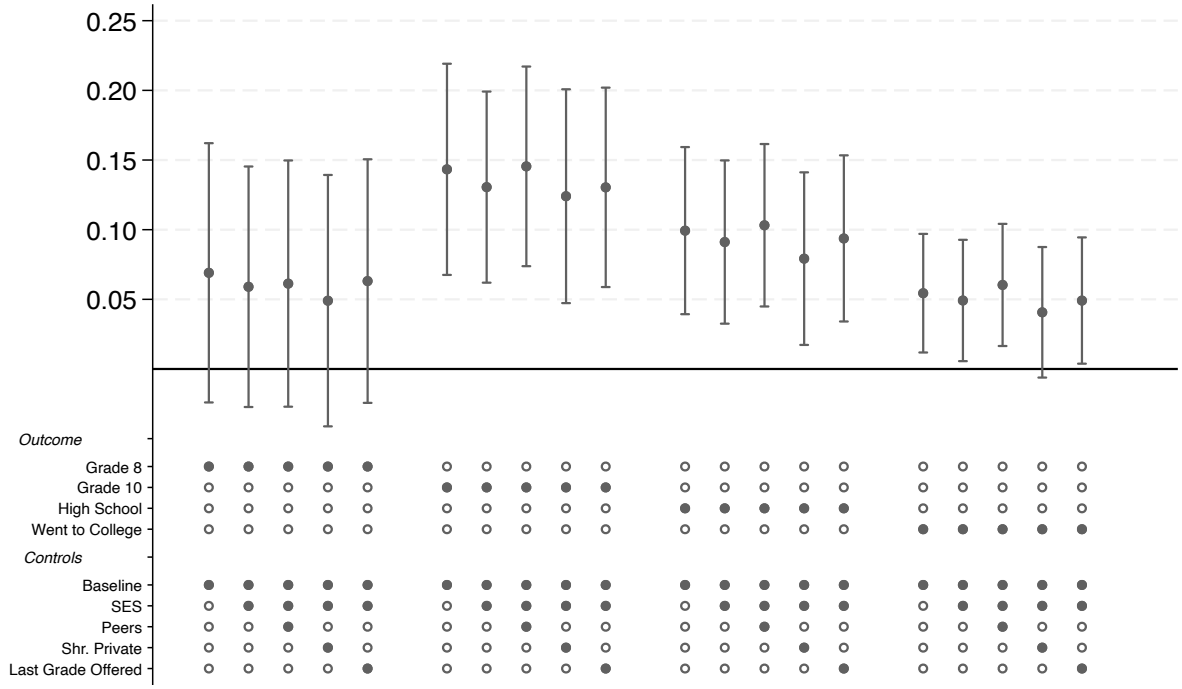
Notes: Binscatter of residualized years of education and residualized mean empirical Bayes SVA after controlling for age fixed effects, gender, and a fixed effect for the first primary school. *Empirical Bayes SVA* is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. Point estimates are displayed in column 3 of Table 1. We trim the fifth lowest and highest percentiles of empirical Bayes SVA for this illustration.

Figure A7: Effects of School Quality on Years of Education by Socioeconomic Status



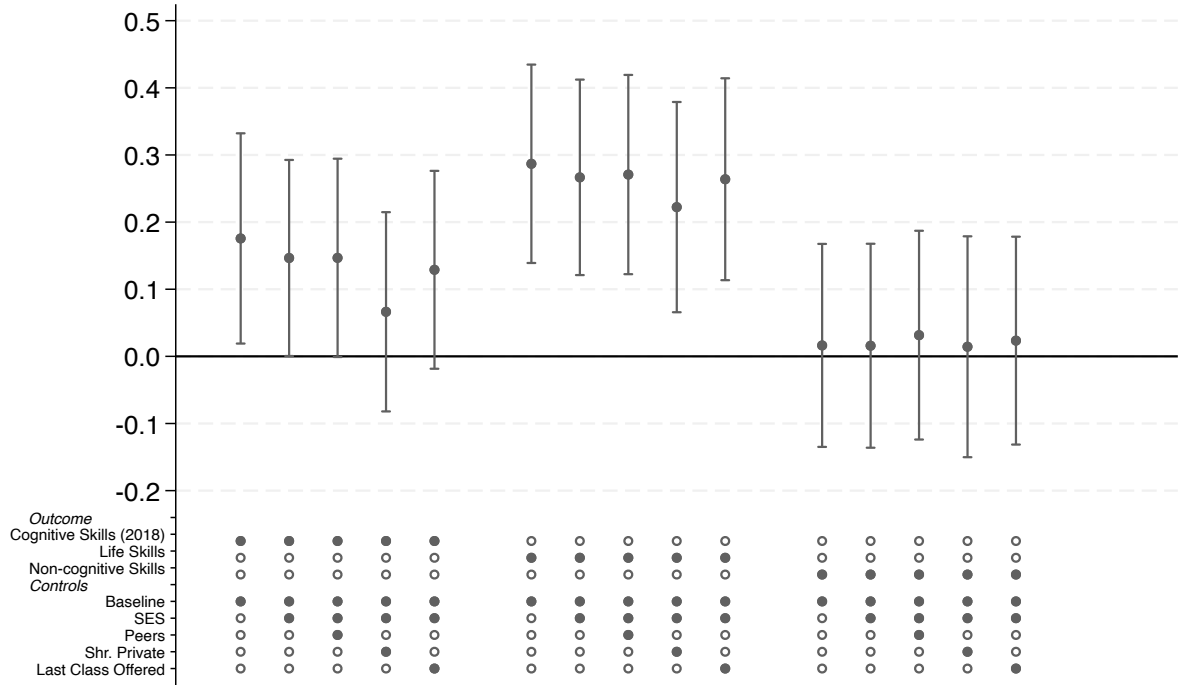
Notes: The dependent variable is the number of years of education. The graph plots the point estimates of the interaction between empirical Bayes school value-added (SVA) and different measures of socioeconomic status. SVA is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. The three measures for socioeconomic status are: having an above-median assets index (first two bars), an indicator variable for mother having some education (middle two bars), and an indicator variable for father having some education (last two bars). The asset index is the first principal component analysis of indicator variables for household asset ownership in 2003. All regressions include age fixed effects, a control for gender, and a fixed effect for the first primary school the individual attended. Standard errors are clustered at the household level.

Figure A8: Tests of Alternative Channels for Different Levels of Educational Attainment



Notes: The graph plots the point estimates of the effect of empirical Bayes school value-added (SVA) on different measures of educational attainment, controlling for alternative channels. SVA is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. The first four rows of bullets displayed at the bottom of the graph indicate the outcome: indicator variables that equal 1 if the individual completed grade 8, grade 10, or high school, or if they attended any postsecondary institution (i.e., *Went to College*). Controls are displayed in the last five rows. Baseline controls include age fixed effects, a control for gender, and a fixed effect for the first primary school the individual attended. Socioeconomic status (SES) controls include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index of households assets (measured in 2003). Controls for missing values are also included. *Peer quality* controls consist of three controls for the average mothers' and fathers' education and average assets of the peers in the school. *Share of private* is the share of time spent in a private primary school. *Last grade offered* is a control for the highest grade offered by the last primary school the individual attended. We group the last grade offered into four categories: the school offers at least some primary levels (grades 1 to 5, the omitted category), middle school (grades 6 to 8), Matric (i.e., grades 9 and 10), and higher secondary (grades 11 and 12). Standard errors are clustered at the household level, and 95 percent level confidence intervals are displayed.

Figure A9: Tests of Alternative Channels for Different Skills



Notes: The graph plots the point estimates of the effect of empirical Bayes school value-added (SVA) on different skills measures, controlling for alternative channels. SVA is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. The first three rows of bullets displayed at the bottom of the graph indicate the outcome: *Cognitive Skills (2018)* is the average score from a test in math, Urdu, and English; *Life Skills* is a score measuring functional literacy and numeracy; and *Non-cognitive Skills* is an index from Danon et al. (2024) measuring socioemotional skills. Controls are displayed in the last five rows. Baseline controls include age fixed effects and a control for gender. Socioeconomic status (SES) controls include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Controls for missing values are also included. *Peer quality* controls consist of three controls for the average mothers' and fathers' education and average assets of the peers in the school. *Share private* is the share of time spent in a private primary school. *Last grade offered* is a control for the highest grade offered by the last primary school the individual attended. We group the last grade offered into four categories: the school offers at least some primary levels (grades 1 to 5, the omitted category), middle school (grades 6 to 8), Matric (i.e., grades 9 and 10), and higher secondary (grades 11 and 12). All regressions include a fixed effect of the first primary school the individual attended. Standard errors are clustered at the first school level, and 95 percent level confidence intervals are displayed.

B Appendix Tables

Table A1: Summary Statistics

	Women		Men	
	(1) Mean	(2) SD	(3) Mean	(4) SD
Age	22.498	2.729	22.790	2.765
High Caste	0.749	0.434	0.709	0.454
Assets	0.344	2.016	0.080	1.937
Mom Has Some Education	0.258	0.437	0.224	0.417
Dad Has Some Education	0.657	0.475	0.596	0.491
Years of Education	8.284	3.704	8.040	3.342
Enrolled	0.154	0.361	0.109	0.312
Went to College	0.141	0.348	0.094	0.292
Works	0.067	0.251	0.764	0.425
Ever Migrated	0.432	0.496	0.449	0.498
Is Married	0.474	0.499	0.256	0.437
Has Children	0.342	0.475	0.158	0.365
Observations	1425		1740	

Notes: This table shows the average characteristics of women and men in our analysis sample. Standard deviations are reported in even columns. High caste, assets, and parental education are measured using the 2003 survey. All other variables are from the 2018-19 long-term follow-up survey.

Table A2: Descriptive Statistics for School Switches

Empirical Bayes School Value-Added	Mean
SVA Before Switch	0.072
SVA After Switch	0.094
Share of Switches by School Types	
Public to Public	0.502
Public to Private	0.140
Private to Public	0.226
Private to Private	0.132
Characteristics of Switches	
Sibling Event	0.795
Sibling Dropout	0.178
Sibling Starts School	0.320
Sibling Switched	0.853
School Closure	0.073
School Entry	0.061
Most Plausible Exogenous Switches	
Excl. Switches Moving w/ Sibling	0.378
W/ Sib. Starting New Sch. Level	0.535
Observations	727

Notes: The top panel reports the mean empirical Bayes school value-added (SVA) of the primary school attended before and after a switch. SVA is in test scores standard deviations. The two middle panels show the proportion of different types of switches in the data. *Sibling Event* is the percent of switches that involve either a sibling dropping, starting, or switching school. *Sibling Dropout*, *Sibling Starts School*, and *Sibling Switched* is the percent of siblings events when a sibling drops school, starts school, or switch school, respectively. *School Closure* and *Entry* are the percentage of switches that involve a school closing or opening. The bottom panel shows the percentage of switches that are the most plausibly exogenous. *Excl. Switches Moving w/ Sibling* are switches that involve a school closure or a sibling entering, dropping, or switching school, excluding switchers when their sibling(s) switch to the same school. *W/ Sib. Starting Sch. Level* are switches that involve a school closure or a sibling entering, or switching school in kashi, grade 1, middle school, or lower or upper secondary school.

Table A3: Characteristics of Primary School Switchers Relative to Stayers

	Assets	Mom Educ.	Dad Educ.	High Caste	Female	First Test Score
	(1)	(2)	(3)	(4)	(5)	(6)
Switched School	-0.059	0.012	0.010	0.031	-0.011	-0.018
	(0.109)	(0.025)	(0.028)	(0.025)	(0.023)	(0.095)
Mean Outcome	-0.010	0.239	0.623	0.727	0.451	-0.422
Observations	3086	3060	2783	3087	3092	926
Clusters	1330	1317	1199	1331	1332	750

Notes: This table shows the characteristics of primary school switchers relative to stayers. The sample is students enrolled in grades 1 to 5. Assets is the first principal component analysis from household assets in 2003. All regressions include a fixed effect for the first school attended. Standard errors are clustered at the household level. Running a regression of switching on the characteristics in columns 1–5 and jointly testing if they are equal to 0 results in an F-statistic (p-value) of 0.895 (p=0.484) with 2,761 observations. If we include first test score, the sample size is 817 observations with a F-statistic = 0.33 (p=0.921).

Table A4: Relationship Between Student Characteristics and Quality of New School

	Dep. Var.: New School's SVA	
	(1) SES	(2) + First Test Scores
Female	-0.075	-0.082
	(0.158)	(0.168)
Assets	0.009	0.012
	(0.026)	(0.026)
High Caste	0.001	-0.008
	(0.134)	(0.131)
Mom some educ.	-0.026	-0.031
	(0.131)	(0.133)
Mom educ. is missing	0.446	0.381
	(0.325)	(0.297)
Dad some educ.	0.018	0.022
	(0.135)	(0.134)
Dad died	0.187	0.210
	(0.164)	(0.180)
Dad is living elsewhere	0.438**	0.396*
	(0.205)	(0.218)
First test score		0.160
		(0.149)
First score is missing		0.045
		(0.145)
F-statistic	1.205	1.414
P-Value	0.296	0.174
Mean Outcome	-0.017	-0.017
Observations	406	406
Clusters	272	272

Notes: This table reports the association between the switchers' characteristics and the empirical Bayes school value-added (SVA) of the new primary school they attend. For students who switch multiple times, the outcome is the empirical Bayes SVA of the first school into which they switch. Assets is the first principal component from a pca analysis of household assets in 2003. Three observations are missing for mother's education, and 11 percent of observations are missing for father's education. Because of the larger set of missings for fathers, we separate the missing indicator variable into indicator variables for missing because the father died and missing because the father is living elsewhere (these categories encompass the full set of reasons father education was not collected). Column 2 includes a control for first test score observed in the data, which is set to 0 when this information is missing, and a control for missing values (75 percent). All regressions include a fixed effect for the first school attended. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: The Effect of School Quality on Attrition

	Is Surveyed		Is Surveyed In-person	
	(1)	(2)	(3)	(4)
	First Sch. FE	+ SES	First Sch. FE	+ SES
Empirical Bayes SVA	-0.025	-0.024	0.008	0.014
	(0.027)	(0.025)	(0.028)	(0.027)
Mean Outcome	0.857	0.857	0.770	0.770
Adjusted R^2	0.329	0.344	0.237	0.245
Observations	3612	3612	3612	3612
Clusters	1533	1533	1533	1533

Notes: The dependent variable is a indicator variable that equals 1 if the individual was surveyed (columns 1–2) or surveyed in person (columns 3–4). The sample is all individuals aged 5 to 15 years old in 2003 who have a measure school value-added (SVA). *Empirical Bayes SVA* is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. All columns include a control for gender, age fixed effects, and a fixed effect of the first primary school the individual attended. Socioeconomic status (SES) controls in even columns include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index of households assets (measured in 2003). Controls for missing values are also included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: The Effect of School Quality on Years of Education, Separating Switches to Better and Worse Schools

	Dep. Var.: Years of Education	
	(1)	(2)
	Baseline	+ SES
Empirical Bayes SVA × Good Switch	0.771*	0.657*
	(0.416)	(0.377)
Empirical Bayes SVA × Bad Switch	0.926**	0.955***
	(0.402)	(0.368)
Empirical Bayes SVA × No Switch	1.013***	0.866***
	(0.277)	(0.252)
P.-val. Equality Good vs. Bad Switch	0.706	0.444
Mean Outcome	8.299	8.299
Adjusted R^2	0.200	0.295
Observations	3092	3092
Clusters	1332	1332

Notes: The dependent variable is the number of years of education. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. *Good Switch* is an indicator variable that equals 1 for individuals switching to a primary school with a higher SVA than the first primary school attended. *Bad Switch* is an indicator variable that equals 1 for individuals switching to a primary school with a lower SVA than the first primary school attended. *No Switch* is an indicator variable for individuals that remain in the same primary school for all the years we observe them in the sample. All columns include fixed effects for age, gender, and first primary school. Socioeconomic status (SES) controls in column 2 include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Controls for missing values are also included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: The Effect of School Quality on Years of Education, Focusing on Most Plausibly Exogenous Switches

	First School Fixed Effect		Excl. Switchers Moving w/ Siblings		Switchers w/ Sib. Starting Sch. Level	
	(1) Baseline	(2) + SES	(3) Baseline	(4) + SES	(5) Baseline	(6) + SES
Empirical Bayes SVA	0.987***	0.867***	0.803*	0.769*	1.241***	1.030**
	(0.283)	(0.256)	(0.468)	(0.410)	(0.461)	(0.435)
Mean Outcome	8.299	8.299	8.275	8.275	8.299	8.299
Adjusted R^2	0.200	0.296	0.199	0.292	0.199	0.299
Observations	3092	3092	2719	2719	2783	2783
Clusters	1332	1332	1258	1258	1266	1266

Notes: The dependent variable is the number of years of education. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. Columns 1–2 show the results from the main empirical strategy (see columns 3–4 of Table 1). Columns 3–4 restrict the sample to non-switchers and switchers whose switches involve a school closure or a sibling entering, dropping, or switching school, excluding switchers when their siblings ever switch to the same school. Columns 5–6 restrict the sample to non-switchers and switchers whose switches involve a school closure or a sibling entering, or switching school in kashi, grade 1, middle school, or lower or upper secondary school. All columns include fixed effects for age, gender, and first primary school. Socioeconomic status (SES) controls in even columns include indicator variables for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index of households assets (measured in 2003). Controls for missing values are also included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: The Effect of Type-Specific School Quality on Years of Education

	First Sch. FE	Gender SVA			First Sch. FE	SES SVA		
	(1)	(2) Own	(3) Other	(4) Both	(5)	(6) Own	(7) Other	(8) Both
Empirical Bayes SVA	1.186***				0.997***			
	(0.392)				(0.311)			
Own Gender Emp. Bayes SVA		0.798**		1.089***				
		(0.367)		(0.378)				
Other Gender Emp. Bayes SVA			0.140	0.709				
			(0.462)	(0.458)				
Own SES Emp. Bayes SVA						0.627**		0.756***
						(0.247)		(0.236)
Other SES Emp. Bayes SVA							0.414*	0.576**
							(0.244)	(0.229)
Mean Outcome	8.871	8.871	8.871	8.871	8.312	8.312	8.312	8.312
Adjusted R^2	0.247	0.245	0.241	0.247	0.202	0.201	0.200	0.203
Observations	1324	1324	1324	1324	3000	3000	3000	3000
Clusters	669	669	669	669	1301	1301	1301	1301

Notes: The dependent variable is the number of years of education completed. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. For *Own Gender Emp. Bayes SVA*, the empirical Bayes school value-added (SVA) is calculated separately for women and men, and a student is assigned the SVA for his/her gender. *Other Gender Emp. Bayes SVA* is then the SVA of the opposite gender. For *Own SES Emp. Bayes SVA*, we first calculate the SVA separately for students from low and high SES backgrounds, separating students into those with a below or above median first principal component from a principal components analysis of household asset ownership indicators in 2003. We apply the same strategy to assign individuals in the long-term sample to low and high SES backgrounds. *Own SES Emp. Bayes SVA* is then the SVA for a student of the same socioeconomic background and *Other SES Emp. Bayes SVA* is the SVA for a student of the opposite socioeconomic background. All empirical Bayes school value-added (SVA) measures are standardized with respect to mean empirical Bayes SVA to allow for a consistent interpretation of the coefficients. All columns control for age, gender, and first primary school fixed effects. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Correlations Between Selected Instruments and Student Characteristics

	(1) Assets	(2) High Caste	(3) Mom Educ.	(4) Dad Educ.	(5) Printed Media	(6) Land Area	(7) High Ability	(8) Household Size
Avg. SVA within 1.2km	0.163	-0.033	0.013	-0.017	-0.009	0.460	0.039	0.234
	(0.300)	(0.068)	(0.042)	(0.075)	(0.033)	(1.172)	(0.036)	(0.521)
Avg. SVA within 1.7km	-0.068	0.045	-0.001	0.064	0.042	-1.274	0.026	-0.986
	(0.306)	(0.060)	(0.040)	(0.083)	(0.039)	(1.292)	(0.050)	(0.770)
Mean Outcome	-0.019	0.727	0.239	0.623	0.091	2.306	0.272	8.773
F-Statistic	0.223	0.313	0.051	0.315	0.639	1.771	1.575	0.821
Observations	3159	3159	3132	2860	3165	3162	3132	3165
Clusters	108	108	108	108	108	108	108	108

Notes: This table reports the correlations between the selected instruments from the lasso instrumental variables strategy and individuals' characteristics. The outcomes are constructed using data from the 2003-04 household survey. All regressions include controls for whether the average empirical Bayes school value-added (SVA) within the two different radii is missing, and village, age, and gender fixed effects, as well as distance to the village center and its interaction with gender. Standard errors are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.